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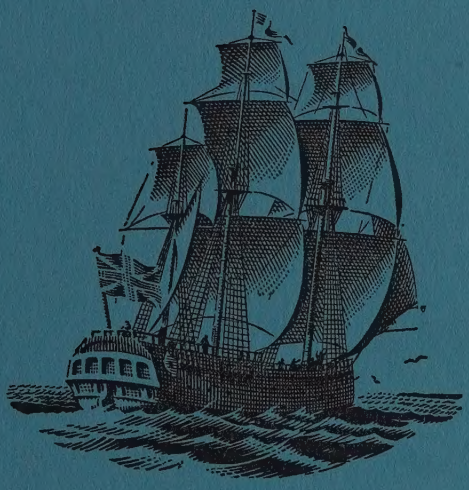

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The drawing on the cover is of the bark Endeavour, *which, commanded by Captain James Cook and carrying a number of scientific workers, was sent out by the British Admiralty in* 1768 *to chart the South Pacific Ocean and observe the transit of Venus*

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ENDEAVOUR

A quarterly review designed to record the progress of the sciences in the service of mankind

VOLUME VIII OCTOBER 1949 NUMBER 32

The scientist in society

That science has become one of the most powerful factors in modern life is a generally accepted, and indeed obvious, fact. The proper role of the scientist himself is, however, a point on which there is no general agreement. On the one hand are those diehards who, ignoring the changed circumstances of the outside world, contend that, outside the laboratory, the personal influence of the scientist should be no more than that of any other citizen. On the other hand are extremists who advocate a state verging on a technocracy, in which scientists would have special privileges and a large measure of control. Those who tend towards the latter view are much more vociferous than their more conservative, and much more numerous, colleagues, with the unfortunate result that there is a widespread impression that scientists generally share these views and wish to claim a far larger share in the control of world affairs than they possess at present. It is therefore timely to attempt an assessment of the proper status of the scientist in modern society.

It is necessary first to consider the particular characteristics which distinguish the scientist from the rest of the community. First and foremost, every scientist has been trained in the rational method of thought known as the scientific method. Rational thought is, however, not his monopoly; it is, for example, acquired also by the legal profession as an essential part of its training. Secondly, he assimilates a fund of technical information which is stored in his mind for immediate application to any problems which may arise. Thirdly, his knowledge of, and ability to interpret, the world's scientific and technical literature gives him the possibility of profiting, through libraries, from virtually all the world's accumulated scientific knowledge, though few have such broad understanding that they can readily grasp branches of science far removed from their own.

In a world increasingly dominated by the application of science such a mental armament must necessarily be formidable, and the scientist is indeed indispensable to modern economy. Yet he is but a link in a long chain, and is no more indispensable than other links which support him. There are administrative and economic factors of which the scientist, with his often circumscribed field of interest, usually knows very little. As we survey the world today, it is clear that its difficulties are due as much to intractable administrative and economic problems as to difficult scientific problems. For example, in the new synthetic antimalarial drugs and the new insecticides deadly to the mosquito, the scientist has certainly provided the means of reducing to small proportions the curse of malaria. Yet it is painfully apparent that it will be many decades at least before malaria is eradicated. Again, the need for improved distribution of existing food supplies between the nations is as great as the need for applying science to produce a sufficient total of food for the world's increasing population.

Though there are scientists who believe that such complex human problems could be solved if only they were allowed to apply the methods of science freely, the majority—and the informed lay public—feel that science is of only limited use in the solution of problems largely influenced by emotional and other imponderable factors. None will deny that such problems as the selection and training of industrial workers are ones which deserve the closest study. The successes of operational research have demonstrated the promising possibilities of science in such fields. Yet such human problems are not to be solved by scientists alone; their solution depends equally on the accumulated wisdom of responsible laymen, who interpret such problems in the light of long and varied experience in human relations, and by reasoning

which—until we know vastly more than we do today about the working of the human mind—we must describe as intuitive.

Experience in attempting to forecast the results of the last American presidential election forcibly demonstrated the present fallibility of applying scientific methods to human affairs. It was no isolated example of failure, nor one surprising to those who have carefully considered the possibilities and limitations of the scientific method. In the event, the calculated estimate of the scientist proved no more reliable than that of the intelligent student of affairs.

To say this is not to decry the efforts of the so-called social scientists, or to try to discourage them from further research. That it may ultimately prove possible to find fundamental laws governing human behaviour is not incredible, but it must be accepted as a fact that precise laws of this kind are yet to be discovered; to assume that any have already been established is most dangerous. In this field above all precision is essential and generalizations are worse than useless. In the case of the presidential election, for example, the plea was made that the computors erred only to the extent of a few per cent. This is perfectly true, but so small a difference may determine the way of life of countless millions. The history of all nations shows that under any political system major events can be engineered by small groups—even by individuals. The actions of such exceptional groups or individuals clearly cannot be predicted by any generalizations applicable to men and women collectively.

We are forced to the conclusion that the scientist alone can at present neither predict nor control the future of the community, nor can he alone be of any great practical benefit to it. Accordingly, scientists have no greater claim to privilege and authority than any other professional group. In particular, the suggestion that because of their particular qualifications scientists should adopt a dictatorial attitude in military matters is under present conditions both foolish and perilous. Such limited experience as the world has had of according privilege and power to scientists suggests that they generally respond no differently from their fellows, and the result can easily be the introduction into science of a dogmatism and intolerance which should be wholly foreign to it.

The need today is not for independent action by scientists, but for very much closer collaboration between them and other specialists. One great obstacle to this collaboration is the terminology of science. Scientists necessarily express their ideas in a form often unintelligible to the layman. Furthermore, the very fact that a man takes to scientific research as his life's work is often an indication of strong individualism, which does not make for easy and tolerant collaboration with others. Consequently, collaboration is not too readily effected even when sincerely sought.

It is often suggested that the gap might be bridged by teaching science more widely in schools, but such a suggestion, while not without value, is far from realistic. It is necessary to accept the fact that the modern world is largely controlled by experts—scientific, political, administrative, educational, and so on. Such experts who acquire some elementary scientific knowledge at school will certainly be the better for it, but they will find such knowledge of extremely little value if in later life they have to discuss scientific matters with qualified scientists.

In science, as in other professions, there is unfortunately no short cut to success. To understand science one must study it assiduously. For the world to make the best use of science there seems to be need of numbers of men of a type which today is scarce though not unknown: those who have had a thorough scientific education, but have also had considerable experience in practical affairs. Only through such liaison officers can the scientific and administrative worlds effectively interchange information and ideas—with immense profit to both—for only men with such qualifications are acceptable to both sides. Rightly or wrongly, the scientist commonly has a profound contempt for the layman who ventures into what he considers his own preserves; equally, the man of affairs feels that the secluded life of many scientists ill qualifies them for giving him any useful advice. A liaison officer of the type suggested would have a foot in each camp and could assist both.

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Bacteriology and chemical kinetics

SIR CYRIL HINSHELWOOD

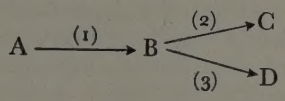
The application of the principles of chemical kinetics to bacteriology has already led to a much fuller comprehension of bacterial activity. Sir Cyril Hinshelwood makes the interesting suggestion that the inheritance of drug resistance and so forth in bacteria may be explained in terms of chemical modifications which persist in the offspring of the modified cells. This conception, he points out, is quite different from the Lamarckian inheritance of acquired characters in general.

It is a commonplace to say that the conventional boundaries of the sciences are rapidly dissolving, and certainly the statement is abundantly illustrated by the relations of chemistry and biology.

Bacteria ought to have a special fascination for chemists, since they perform amazing chemical feats without any of the apparatus in the way of the special organs with which higher plants and animals are provided. They are unicellular, and such internal structure as they possess is so fine-grained and delicate that special methods are needed to reveal it. There are internal inhomogeneities which can be revealed by staining techniques (though these are sometimes suspected of modifying the appearances they are intended to reveal), or by phase-contrast microscopy. Some parts of the cell material are fairly generally supposed to be analogous to the nuclei of other cells, and to undergo cyclical changes in distribution. Bacteria, however, do not possess the obvious chromosomes of some cells, and there is no generally accepted evidence of a sexual process of reproduction.

Many of them grow when provided with such simple constituents as glycerol, ammonia, and a few inorganic salts. In the space of about half an hour each cell synthesizes all the necessary proteins, nucleic acids, and other macro-molecular compounds required to reduplicate itself. It then divides into two by a process which may to some extent be accelerated or retarded independently of the actual rate of increase of mass, so that changes of cell size occur. The growth process must involve a series of polycondensation reactions, in the consideration of which chemical kinetics surely has something to say.

Bacteria indeed show many phenomena which excite the interest of the physical chemist. It might at first sight be supposed that the sequences and combinations of chemical reactions taking place in them would be too complex to be sorted out in the kind of way in which physical chemists try to analyse simpler systems. This, however, is not so. General propositions about groups of linked chemical reactions can be correlated with general facts about bacterial behaviour. An example will perhaps make this point clearer. Suppose we have a system of competing reactions on the following general plan:



Acceleration of process (2) will deplete the supply of B and therefore adversely influence the rate of process (3).

The following group of observations does in fact seem to be based upon the kinetics of just such sets of competing reactions:

(*a*) Certain cells will multiply readily in presence of sodium nitrate as a source of nitrogen. If an ammonium salt is added to the medium it is at once utilized, and reduction of the nitrate ceases abruptly.

(*b*) Coliform bacteria normally use atmospheric oxygen. They can adjust their economy to derive the oxygen from nitrates. If, however, a culture which is reducing nitrate is suddenly aerated, reduction of the nitrate stops. Evidently the oxygen removes an intermediate which is required for reaction with the nitrate. Further adjustments may, however, occur slowly and bring about the result that nitrate may be reduced even in presence of oxygen.

(*c*) Some bacteria will grow both aerobically and anaerobically, and these show the well-known Pasteur effect. Anaerobically, they bring about disproportionations of carbon compounds. In presence of oxygen, not only does profound oxidative breakdown occur but the disproportionation reactions are actually inhibited. Agents which would cause reduction of the carbon substrate are

removed too rapidly by oxygen to exert the effects which they could exert in its absence. The point about these phenomena is this: they are evidently explicable in terms of an interference with one reaction by a competing reaction which consumes a common intermediate at so great a rate that its rival is inhibited.

Now there should be other groups of facts which also can be correlated in terms of what might be called general propositions in chemical kinetics. The complexity of the machinery of cell growth being essentially chemical rather than mechanical, it is evident that the vast series of consecutive and competing reactions can be interfered with and modified in numerous kinds of way. One way which is obviously possible, and which strikes the chemist as being important, is that the relative velocities in a series of consecutive reactions can be changed. The peculiar importance of this is as follows.

When a cell grows and reproduces itself, there occurs an elaborate series of consecutive reactions leading to the synthesis of all the various parts of the cell material in what, under constant conditions, are constant proportions. Every part of the material, since it is in fact reduplicated at each cell division, is effectively autosynthetic, though the autosynthetic character must depend upon the mutual interaction of groups of enzymes and cannot be an inherent property of all enzymatic material. Growth depends upon a series of enzyme processes in which the products of one process constitute the raw materials for the next, and are thus active intermediates in a sort of continuous production line.

When the conditions of growth are changed, for example by the provision of a new source of carbon or nitrogen or by the addition of drugs to the medium, the relative velocities in this chain of processes may very well be rather profoundly modified. If this is so, some parts of the cell material may be synthesized more rapidly than others, and the actual proportions in which the various constituents are present when the moment comes for division will be changed. The result of the division process will be a cell in which the chemical and material balance is different.

This elementary notion at once attracts the attention of the chemist to the extraordinary adaptive phenomena exhibited by bacteria.

Bacteria are extremely variable, though it must be emphasized that the variability manifests itself only within definite limits. One bacterium never changes into another of completely different type, but within a certain amplitude its properties vary very considerably. Some of the types of variability will be briefly indicated.

(*a*) The cells may normally refuse to utilize a given carbon substrate, but after remaining in contact with it for a time they may grow, at first slowly, and then at an increasing rate, until finally they use it with optimum efficiency. For example, *Bact. coli mutabile*, which could grow immediately with glucose as carbon source, waits several days before it will touch lactose, which it then consumes. After a little 'training' it continues to consume lactose as well as it could glucose.

(*b*) Bacteria placed in presence of inhibitory substances, at concentrations just insufficient to suppress growth entirely, multiply with extreme difficulty at first, but after a time acquire the power of growing as though the toxic agents were not present at all. They are said to have acquired drug resistance.

(*c*) When they have been trained to use new sources of material or to resist drugs, bacteria not infrequently show limited but quite definite changes in biochemical properties, such as enzyme activity in particular directions.

(*d*) When exposed to short-wave radiation, such as ultra-violet light, bacteria which are not killed outright may show changed properties. The change is usually such that the cell has lost some of its initial chemical potentialities. For example, whereas initial growth might have occurred quite readily with ammonium salts as the sole source of nitrogen, after the irradiation the use of this simple source for synthesis becomes very difficult, although growth with utilization of suitable amino-acids remains easy. Evidently the ultra-violet light damages some part of the synthetic mechanism. It is to be noted, however, that this can repair itself automatically in the course of a process whereby the cells are left in contact with a medium containing ammonium salts, which they become re-trained to utilize.

It is attractive to attempt to make a working hypothesis in which phenomena such as these are interpreted by applications of chemical kinetics. There is not space to discuss the matter in any detail, but a simple example of what is meant may be quoted.

One very natural way in which a drug can retard part of the autosynthesis is to cut off the supply of an active intermediate by interfering with the operation of the enzyme which provides it. Now it is reasonable to imagine that there are certain essential parts of the cell structure, and

that until all these have been built up in some critical amount there will be no cell division. If the growth of some essential part is inhibited by the action of the drug, and if division waits until this part attains its normal critical size, then it is clear that in the meantime other parts of the cell, not inhibited by the drug, will often be built up in more than the normal amount. These 'expanded enzymes' include those which provide the active intermediates for the systems inhibited by the drug. The active intermediates can now be formed in more than the normal concentration, and will thus be able to antagonize the action of the drug on the inhibited enzymes.

Examination of the differential equations for the consecutive reactions occurring in a very simple model based upon the above idea shows that the enzyme proportions in the cell may continue to change until the growth rate attains the normal value in the drug-free medium. This constitutes an automatic development of drug adaptation.

In a very crude way this notion is illustrated in the diagram. In row 1 (*a*) and (*b*) are two enzyme systems which increase in step until there is enough of each for the cell to divide as indicated. The intensity of the shading represents the concentration of the active intermediate which (*a*) produces and (*b*) utilizes. In row 2 the inhibitory action of the drug is supposed to be exercised on the production of the intermediate by (*a*). There is a reduced concentration, so that by the time (*a*) has attained the state normally corresponding to cell division (*b*) is lagging behind. Division waits until (*b*) has reached the appropriate state (third diagram of second row). By this time (*a*) is present in so much greater amount than normal that the concentration of the intermediate has been restored. Division then results, with the production of two cells each containing subnormal amounts of (*b*). The third row represents the behaviour of the adapted cell in presence of the drug. The increased amount of (*a*) gives an extra supply of intermediate which can compete effectively with the drug and allow the normal growth of (*b*). This picture is of course grossly oversimplified, but it seems hard to believe that phenomena of this general kind do not occur, and that they do not have their significance in adaptive processes.

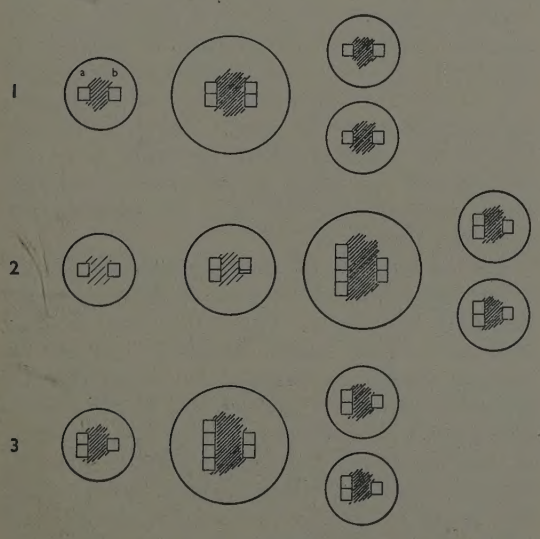


FIGURE 1 – *Diagram to illustrate the automatic development of drug adaptation.*

Indeed, a rather more general study of the differential equations of consecutive reactions by which a series of enzyme systems is built up in a co-ordinated manner shows that the system as a whole will (on some quite simple and plausible assumptions) often have the property of so adjusting the proportions of its various parts that an optimum overall growth-rate will be established. This clearly has considerable significance in connexion with the adaptation of bacteria to growth under new conditions generally, and in particular with the utilization of fresh sources of material.

The assumptions underlying these ideas are not very specific or very arbitrary. Indeed they are little more than formulations of obvious experimental facts. Bacterial cells can grow by the utilization of the most varied substances. According, therefore, to the medium in which they multiply they must utilize different reaction routes. The combinations of possible routes constitute a kind of railway network. Some parts of the possible network will be slow, some rapid. For the total complex series of simultaneous and successive chemical changes there will be, for a given medium, one route through the network which can be traversed more rapidly than any other. This quick route will be different for different media. This is the first principle.

The second principle is as follows. When the cells grow, every single enzyme system is reproduced. ('Enzyme system' is here used in the widest sense to include what are variously called enzyme precursors, plasmogenes, co-enzymes, and so on.) It is very clear that the reproduction of the enzyme systems is closely linked with their functioning, since growth cannot occur without the elaborate functioning of all. Therefore, when growth by a

given chemical route occurs, it is almost certain to be accompanied by a preferential building up of precisely those parts of the enzyme system which are most in use. There is an obvious physical hypothesis about this, namely that the synthesis of proteins and other compounds is guided by existing structures in a manner analogous to crystal growth, but, as far as kinetic consequences are concerned, one can regard the correlation of function and autosynthesis independently of any detailed picture.

Such ideas have been criticized as lacking demonstrative significance. The criticism misunderstands the purpose of the argument, which is not to explain autosynthesis, but to show that the fact of autosynthesis demands changes in enzyme proportions according to the circumstances of growth. These changes in proportions would have the clearest connexion with adaptive processes. If the utilization of a new substrate demands the functioning of an enzyme present initially in minute proportion, then growth will be initially difficult. But there seems very good reason to believe that, if this enzyme can be reproduced at all, then it will increase in proportion as expansion of the total material takes place in the new medium.

The hypothesis about adaptation just discussed envisages a change in the proportions of different kinds of enzyme material. Other kinds of changes which must be considered are those in the actual composition of the macro-molecular constituents themselves, and in the mode of folding and disposition of their structures. These factors are clearly subject to influence by alteration of substrates and so on, in a way to which kinetic considerations are relevant.

It might have been supposed that the construction of models of this kind would be regarded as reasonable hypotheses of the chemist, to be judged as helpful or unhelpful and pursued or discouraged accordingly. But here we run into stormy weather. The changes which accompany the development of drug resistance, or the acquisition by the cells of the capacity to utilize a new source of material, are often transmissible to the progeny formed by the division of the original cells. This has led some people to take the following standpoint. Drug resistance, to take a typical example, is, they say, an acquired character, and therefore could not possibly be inherited. To suppose that it could, would be 'Lamarckian' and therefore heretical in the extreme, rendering those who entertained such a chemical working hypothesis about bacterial affairs liable to condemnation without trial.

Let us examine briefly the logic of this position. Lamarck supposed that changes produced in an entire animal by its mode of life could be transmitted to the offspring. For example, the children of a blacksmith might have unusually muscular arms.

Of course they do not, and the reason is sufficiently clear. The changes brought about in the blacksmith's arms do not affect the hereditary material. In the same way, even the blacksmith's addiction to a drug would be rather unlikely to affect the genes he transmitted. If, however, something were done to him which actively changed the germ plasm, then inheritable effects might well occur. The failure of what can legitimately be called the Lamarckian idea is clearly due to two things: first, that in complex organisms environmental influences, chemical or otherwise, are extremely unlikely to have a chance to act upon the germ plasm, and secondly, even if they do, they are still sufficiently unlikely to do anything significant to it, between killing it outright and leaving it alone. Thus it would appear that environmental effects on heredity might in principle appear in rare cases, but that the chance of producing any by conscious effort is very small. What the uncontrolled effects of cosmic rays and the like may be no one at the moment is really in a position to say.

With unicellular organisms, however, the position is surely very different. As we have seen, changes in the proportions and even in the structure of the self-duplicating material are possible in principle as a result of changed relative reaction rates. The cell produces its progeny by simple fission, so the considerations which explain why acquired characters of intact organisms are not passed on have no weight. Painting the outside of the building where the bank notes are printed will not affect the appearance of these notes: pouring nitric acid on to the printing press itself may well do so.

But the rejection of illogical prejudices against a working hypothesis does not in the least establish that hypothesis. There is a well defined alternative view about adaptive changes in bacteria themselves: namely, that of the selection of chance variants. This will now be considered. The point I have wanted to make first, however, is that the choice between hypotheses depends upon detailed and rather intricate technicalities, and is not in any way assisted by slogans and labels, whether applied in condemnation or in approval.

Having made this point as clear as may be, let us examine the pattern of a typical adaptive change, namely the development of drug resistance.

On culture of the cells in presence of graded amounts of the drug the resistance develops rapidly. In general, it does not appear except in so far as new material is actually synthesized. As soon as the cells are able to grow normally in presence of the drug they are said to be trained. When the training is just complete it is unstable, in the sense that if the bacteria are returned to a drug-free medium and allowed to multiply in it for a season they revert and lose their resistance. If, however, they are trained by being allowed to multiply for a much more prolonged period in presence of the drug, then the adaptation becomes stably impressed upon them, and they will retain their resistance during many generations of growth in the complete absence of drug to keep them, as it were, in practice. Actually, it is more correct to speak of metastability than of stability, since under appropriate treatment (growth in various unusual circumstances) they will lose their resistance and revert. For example, certain cells trained to resist sulphonamides lose their adaptation when grown in presence of acridine derivatives.

If these facts are interpreted in terms of selection, then there are two possibilities. The first is that the original strain was heterogeneous, and contained inherently drug-resistant types which multiply differentially in presence of the drug. This can be definitely disproved, since all kinds of adaptive phenomena manifest themselves with strains which were originally derived from single cells. The second is that chance mutations are continually occurring to produce strains with every conceivable type of property, and that the most suitable are selected out in the course of the training. Unstable training would be characteristic of an impure resistant strain, stable training would be attained when the resistant strain had been purified by long enough continued selection. Ultimate reversion under abnormal conditions would have to be ascribed to a 'back mutation.'

From here the argument, I think, develops along lines which will not be very welcome to those who like to manufacture clear-cut ideological issues. We begin by asking in what the chance mutations consist. Crude actions such as the chemical effects of cosmic rays are not very satisfactory to postulate, since innumerable forms of adaptation occur, and many kinds can be superposed one upon the other. Thus the mutational changes must be, as it were, of a fine-grained and subtle texture. Permutations and combinations of not too small numbers of genes would provide an answer, but where are these to be found, and how are they to be redistributed, in a non-sexual unicellular organism with only fine-grained structure? It does not seem too much to suppose that a quite considerable proportion of the bacterial mass itself possesses genic character, nor does it seem out of the question that the 'genic' character of a bacterial cell might be a function of the quantitative proportions of its components, or of the precise chemical composition of its macromolecules, or of the folding of its proteins, all or any of these factors being closely related to the configuration of its nucleic acids.

All that the chemical theory suggests is that changes in these are forced upon the cell by changed relative reaction rates prevailing when autosynthesis occurs under changed conditions, and that in unicellular organisms not everything is governed by purely combinatory processes. Even here, of course, it may be said that a chemical change is itself only a combinatory change of atoms, so that in the last resort the question at issue is one of scale only. In a sexually reproducing organism, where genes are carried in chromosomes, one can see that the grosser combinatory changes are of major importance, and that chemical changes in genic material likely to be rare. Bacteria lie at the other extreme of a spectrum. It is difficult to see how gross combinatory changes can be of outstanding importance except on a scale where they are describable in the terms used above. Chance development of drug resistance may of course occur: it seems more probable that the response develops as a result of the changed reaction rates in the way outlined earlier. Indeed, if this possibility is denied, it would seem incumbent upon us to find a positive reason why so simple and obvious a mechanism should be excluded.

There remains the problem of why chemically induced changes should be stable in the absence of active disturbance. Two factors would contribute. First, the pattern of existing material in certain key parts of the cell may well guide the deposition of fresh material in a manner somewhat analogous to the growth of crystals. Secondly, if an enzyme has increased in amount to produce, say, excess of some intermediate to combat the action of an inhibitor, when the latter has been removed this excess of intermediate does not need

to provoke a reversion, as may easily be shown by a simple quantitative calculation.

What has been said has tended to blur the sharp distinction between the mutational and the adaptive view of cell changes, and this I believe is right. But there is one respect in which, at first sight at least, a rather important difference exists between the selection view and the view that adaptive response may occur as a result of changed reaction rates. In one case the change is initiated in very few cells, which presently outgrow all the rest; in the other case it affects the bulk of the cell population more or less uniformly. Here again, the final answer will probably prove to be that there are types of behaviour varying between two extremes. In some cases, however, it seems definite enough that the modification occurs more or less equally in all the cells. For one thing, changes in a property of a bacterial population do in fact sometimes occur under conditions where it can be directly shown that the two extreme 'types' have exactly the same growth rate and so would not suffer differential increase.

Another kind of evidence is illustrated by an example which can be given in slightly more detail. Sometimes bacteria placed in contact with a new sugar, such as lactose, refuse to grow for several days, after which they will multiply well enough. The two views are: (1) The original population contained only a very minute fraction of variants capable of growing at all and the delay represents the time which they require to multiply up to a number detectable in comparison with the large number which do not multiply. (2) They all take a long time over growth in the first instance because they lack the necessary intermediates to utilize the unaccustomed source. The delay represents the time during which enzyme systems not normally much used elaborate these intermediates. In the course of the subsequent growth by the new mechanism these enzymes undergo an automatic expansion whereby training occurs. In point of fact, it has been shown that, in some cases at least, practically every cell of the original sample plated out on the new medium (in a solid form to allow counting of colonies) does in fact grow after the lapse of the requisite time, and the same conclusion may be reached by another technique for liquid media.

Suppose dilutions down to 10^{-8} of a given culture grow on transfer to a fresh glucose medium. The last dilution to give positive results may be presumed to contain about one single living cell. Suppose further that only 1 in 10^5 of the original cells are, say, '*d*-arabinose variants.' The last dilution to show growth on transfer to *d*-arabinose would be about 10^{-3}. On the other hand, if all the original cells were capable of adaptation, then positive results would be found down to the 10^{-8} dilution, but growth would require much longer. In some cases at least the second possibility is realized.

Thus in certain examples the direct adaptation does occur, and does apparently represent the establishment of a new chemical growth-sequence.

Once again, however, apparently sharp issues become blurred. When the new growth-sequence is established, certain enzymes originally present in small amount per cell increase relatively to the others. This might in a sense be called an internal selection.

In some cases, no doubt the required enzymes or modified genic material might be confined to a few fortunate cells. There would then be external selection, but in both cases a relative change of proportions occur.

There are very many other ways in which chemical working hypotheses may possibly throw light upon cell working: the study of oxidation-reduction relationships in connexion with aerobic and anaerobic growth rates, the study of substances which accelerate or retard division, and innumerable other matters. But the chemist must realize that he uses very crude tentative models of very complex affairs and should be modest in his conclusions, while perhaps some biologists have need to discard a few prejudices.

Autumn colours

W. H. PEARSALL

Autumn, the 'season of mists and mellow fruitfulness,' is marked by a vivid display of leaf-colour which goes far to allay regrets for the summer that is past. Professor Pearsall explains how and why the autumnal colours are developed; he shows that the changes are intimately connected with the seasonal life-cycle, and relates them to other colour-changes in plants.

The autumn colours of leaves mark the end of the annual growth-cycle of the leaves that show them. Each leaf has passed through three stages: one in spring, when it was engaged in building up its own living material and structure; a second and longer one through the summer, during which it was manufacturing foods for the rest of the plant; and a third stage marked by autumn colorations, in which valuable materials present in the leaves are being removed for storage.

The processes leading to the appearance of the autumnal colours are obviously associated with the death of the leaf, and it is not a difficult matter to show that they are also associated with the evacuation of materials from the leaf, particularly of those substances which are, in fact, valuable for the initiation of further growth in the following spring. It is not necessary, however, to suppose that this is purposive, but merely that the balance between various processes so changes that this is the result. During most of the summer, the removal of such substances as protein and starch from the leaf on the whole balances the rate at which these materials are manufactured, so that the overall changes are small. As the leaf becomes old, however, the rates of synthesis fall off while the breakdown processes are accelerated, a natural internal change that is much accelerated by the less favourable conditions in autumn. It leads to the production of soluble substances like sugars, which are easily transported from the leaf elsewhere.

It is evident from superficial observations that the changes in colour generally follow a definite order. The first sign of change is often that the green leaf shows a tendency to become red (figure 9). There may, at this stage, be no signs of yellowing (figure 12). In many plants, however, reddening is associated at a slightly later stage with the development of the normal yellow colour (figure 7). In any case, a stage is reached at which yellow is one of the predominant colours (figure 1), and shortly after this the leaf becomes brown (figure 2). This final stage is associated with the death of the cells and with the drying of the tissues as a whole.

There are many leaves, however, which do not produce a red colour. The beech (*Fagus sylvatica*, figure 2), the plane-tree of London squares (*Platanus acerifolia*, figure 11), and the maidenhair-tree (figure 6), as well as native English Oaks (*Quercus robur* and *Q. petraia*) are all species of this type. The yellow colours occur in all plants to a more or less marked degree. They are most evident in trees of the temperate climates simply because these are strongly seasonal. In tropical forests, where the climate varies little with the seasons, the trees show both leaf-growth and leaf-fall at almost any time. Similarly evergreens, like pines and firs, are continually producing and losing leaves, though each leaf may stay on the plant for two or three years. As only a few leaves change colour at any one time, the effects are not noticeable. The few trees of this group, the Gymnosperms, that produce annual crops of leaves each spring show well-marked autumnal colour changes (*cf.* figure 6).

The actual sequence of events thus depends upon the capacity of leaves of different species of plants to produce colours. The colours belong to three main groups. The normal green colour of a leaf is due to the presence of *chlorophylls*, complex green pigments containing the metal magnesium, which are intimately concerned with the normal synthetic functions of the leaf. Besides these green pigments, however, a leaf also normally contains two sorts of yellow pigment known as *xanthophylls* and *carotenes*. The former are also present in many yellow flowers, such as daffodils, while the latter are mainly responsible for the colour of carrot roots. All these green and yellow pigments are insoluble in water and can normally be removed from the leaves only by using solvents such as acetone, alcohols, or chloroform. In addition to these pigments, some plants may also contain

FIGURE 1 – Liriodendron tulipifera (*Tulip tree*).

FIGURE 2 – Fagus sylvatica (*Beech*).

FIGURE 3 – Prunus serrulata.

FIGURE 4 – Nyssa sylvatica.

FIGURE 5 –
Sorbus domestica.

FIGURE 6 – Ginkgo biloba (*Maidenhair-tree*).

FIGURE 7 –
Parrotia persica.

FIGURE 8 – Sorbus aria (*White Beam*).

FIGURE 9 – Acer nicoense—*Japan.*

FIGURE 10 – Quercus coccinea.

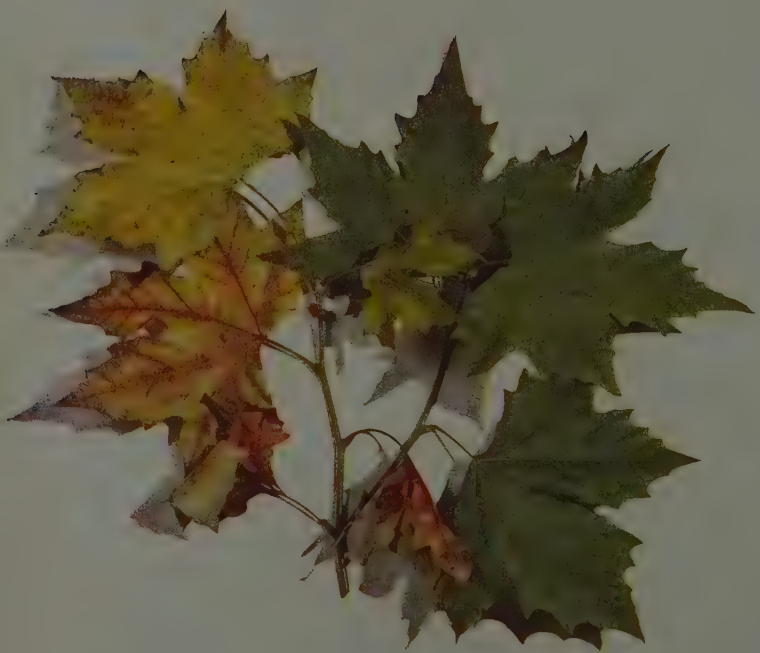


FIGURE 11 – Platanus acerifolia, *showing acceleration of yellowing (on left) caused by treatment with coal-gas.*

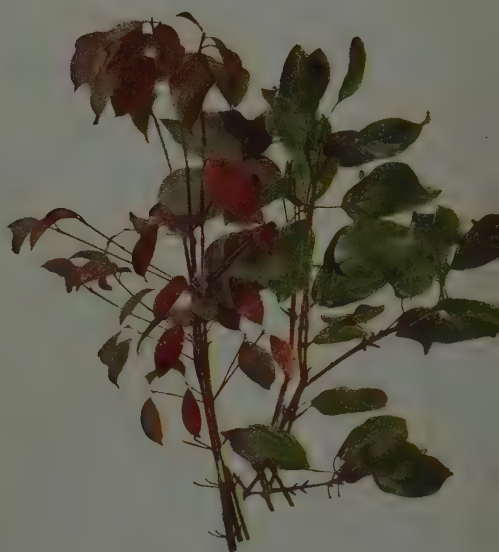


FIGURE 12 – Cornus sanguinea, *showing red coloration produced by ringing the shoots on the left.*

red pigments that are soluble in water and so are present throughout the aqueous cell liquids, e.g. the red colouring-matter of beetroot. These are *anthocyanins*; they are not universally present in plants, and even species which can make them do not always do so. When they occur, their presence is associated particularly with the appearance in the cells of large amounts of sugar. There is no pigment normally present in leaves which produces a brown colour. This is the result of post-mortem changes—oxidations akin to the browning produced when a cut apple or potato is exposed to air. Otherwise, the colours which appear in autumn are those due to combinations, or to a local predominance, of the normal pigments.

It has already been pointed out that, from the point of view of the plant functions, autumn colours are associated with the process of evacuation of valuable materials. Chemical analysis shows that this process is generally started by the transformation of starch (an insoluble food reserve) into sugars. This is the stage at which the red colours (anthocyanins) may first appear, for, as already noted, their formation is generally correlated with a high sugar content. The next stage is marked both by the removal of the last reserves of carbonaceous or sugary foods and at the same time by the mobilization and removal of the green pigments. The leaves then go yellow because the yellow pigments become predominant, not because new colours are formed, and this fact is well illustrated by the following figures for the pigments present in sycamore leaves during the process of yellowing:

Pigments present in yellowing sycamore leaves

Days	*Colour of leaf*	*In per cent. of original amounts*		
		Chlorophyll	*Xanthophyll*	*Carotene*
0	Green ..	100	100	100
3	Light green	59	89	100
5	Yellow ..	16	72	95
7	Brown ..	2	45	63

During yellowing, the final reserves of substances capable of yielding the sugars necessary for anthocyanin formation are now converted into sugar, so that here is a second—and also a last—opportunity for the leaf to become red. After this stage the leaf is finally depleted both of its food reserves and of the pigments necessary to manufacture more food. It quickly dies, and at the same time dries, turning brown.

While these colour changes follow a more or less uniform pattern they show considerable differences in intensity, which can be traced to differences in the internal or external conditions. The beautiful autumnal effects seen in our English woodlands would pale into insignificance if put alongside the flaming colours of many North American forests. The difference is, of course, partly due to the peculiarities of the trees; both maples (*cf.* figure 9, which is not, however, North American) and also oaks, like *Quercus coccinea* (which is often planted in Britain, figure 10) contribute to it, as well as some of the shrubs (*cf.* figure 4). But there are also differences due to the temperature conditions. The North American 'Indian summer,' when the colours are at their best, is usually markedly bright with frosty nights, conditions which favour the production of red pigments. In like manner, it can be noticed in Britain that a frosty autumn with bright days between the frosts almost always gives much better colours—in particular, reds—than one observes during a wet autumn, when dull yellows and dirty browns predominate. These differences are clearly due to environmental influences. The rapid appearance of dull yellows and browns in a wet autumn is like the similar effect produced when greenhouse plants are too frequently watered. This treatment removes various soluble substances from the leaves, especially potassium salts, which appear to be associated with the normal water-retaining capacity of a leaf. Their removal is associated with the early drying and death of the leaves, which may even precede the usual yellowing changes.

For the explanation of these other colour effects we have to look to the effects of temperature upon sugar accumulation, and hence upon the resultant anthocyanin formation. At low temperatures, near the freezing point, insoluble substances like starch are very rapidly converted into sugar, although the rate at which the sugars can leave the leaf is decreased by low temperatures. The resultant accumulation of the sugar leads to red pigment formation. A similar effect is obtained when potatoes are exposed to temperatures near the freezing point, for after this treatment they become sweet to the taste. Trees, moreover, show a similar effect in spring when the starch stored in the trunks is being converted to sugar. The sugar maple (*Acer saccharum*) gives, therefore, much larger yields of sugary sap after cold nights.

Any other treatment which leads to accumulation of sugar in the leaves is liable also to cause the production of red colours in autumn. Experimentally, sugar can often be made to accumulate by preventing its transport from a leaf or leafy shoot. One way of doing this is to 'ring' the shoot, that is, to scrape off all the soft outer tissues through which most of the sugar transport takes place. When this is done in autumn, sugar necessarily accumulates in the leaves and they turn red if suitable plants are used. Twigs of dogwood (*Cornus sanguinea*) treated in this way are shown in figure 12, along with untreated twigs of similar origin. Privet, especially the wild forms, and elder are also useful for this purpose, but plants which do not normally form red pigments are useless (e.g. the British oaks and beech).

It is sometimes possible to produce the same effect (if the twigs are not too brittle) by bending back a branch, so as to constrict the conducting tissues and prevent sugar transport. In North American sugar maples, sugar accumulation and red colour production can be increased by fastening branches with the tip vertically downwards, but this does not seem to be successful with our English sycamore (*A. pseudoplatanus*), which forms red pigments less easily.

The yellowing changes are also affected by external conditions. A leaf can be induced by suitable treatment to turn yellow at almost any time. The simplest way of accomplishing this is to keep it in darkness for some days, and if this is done with a series of leaves of different ages, it will be found that the old ones yellow first, while the young ones are comparatively resistant to the onset of yellowing changes. The same thing happens during the autumn colour changes. Young leaves, that is those nearest the stem apex, are generally the last to change colour, and this is shown in the leaves of the tulip-tree, *Liriodendron tulipifera*, illustrated in figure 1.

Simple experiments like cutting through a vein will usually retard the onset of yellowing in part of a leaf, merely because this treatment prevents the soluble substances which are formed from being removed. We learn thus that the veins in a leaf are the actual means of transport, although some movement may take place through the intervening tissues. The distribution of autumn colours is usually in accordance with this, for the yellowing changes are normally most rapid along the veins and often occur first in the basal leaflets, from which evacuation is most easy. Drying, and the resultant production of brown colours may appear first at the edge of the leaf and between the veins, though they do not do so in any of the examples figured here.

Yellowing is also more rapid if the leaves remain attached to some part of the plant that is using the materials transported away from the leaf. Thus leaves on an actively growing stem, or on one bearing developing fruits, will turn yellow more quickly than similar leaves detached and kept with their stalks in water.

Finally, the yellowing changes are also dependent upon other conditions as well as upon the presence or absence of light. One of the curious effects of external conditions can still be seen occasionally near street gas-lamps, though it is less obvious nowadays than it was in the days when old-fashioned jet-burners were employed. It was noticed at that time that the leaves turned yellow and died on trees near street lamps much more quickly than they did when growing some distance away from them. This was finally traced to the presence of unsaturated hydrocarbons like acetylene and ethylene, which were present in the coal gas. These substances markedly accelerate the changes which lead to yellowing. Thus of similar samples of leaves, those kept in air containing coal-gas (or acetylene or ethylene) will yellow more quickly, as is shown in figure 11. Ripening fruits like green bananas can also be made to yellow more quickly by this method. It is a curious fact that some fruits, apples for example, give off small quantities of compounds such as ethylene, so that the presence of one fruit may accelerate the yellowing or 'ripening' of another. This is one of the reasons why it is advantageous to wrap up stored apples in paper. The fact is interesting also because it shows that the ripening of a fruit, though proceeding far more slowly, is in many respects analogous to the yellowing of a leaf, so that the information we can obtain about one of these processes is very often capable of being applied to the other. Both are interesting as examples of that mysterious sequence of events we call senescence.

The *Opus maius* of Roger Bacon

A. C. CROMBIE

The *Opus maius*, and the *Opus minus* and *Opus tertium* which may be regarded as appendices to it, comprise the essential parts of Roger Bacon's contribution to science. This celebrated Somerset friar has always proved somewhat of an enigma to historians—whether of science or not—and little agreement has been reached in estimating how far he possessed the true spirit of science. Dr Crombie makes an important contribution to the discussion.

Since his death (probably in the year 1292) Roger Bacon's reputation has passed through various phases, each of which represents a different way in which he has caught the imagination of mankind. Almost from the beginning there has been a Roger Bacon of legend as well as of fact[1], and quite apart from his actual contributions to science he has been used by different ages as a symbol for their own beliefs and aspirations. For the 16th century, as for the 14th, Roger Bacon was a magician, who attempted to gain power over nature and mankind by tapping occult and dangerous forces, who had made a monstrous brazen head:

'That, by the inchaunting forces of the Devil,
Shall tell out strange and uncoth Aphorismes,
And girt faire *England* with a wall of brasse.'

For the 19th century he was again a symbol of the desire for power over nature, but then he was thought of as a 19th century scientist born before his time, impatiently trying to break through the web of metaphysical pseudo-problems which entangled his contemporaries—in fact as an early positivist. More recently, historical scholarship has made it possible to deny that

'The fame of *Bacon*, bruted through the world,
Shall end and perish with this deepe disgrace,'

but has also created a further legend: that he deserves no reputation at all beyond that of a querulous nuisance.

The whole truth about Roger Bacon's position in the history of science is something much more complex than any of these legends, and can be understood only by trying to discover the problems with which he was faced and the particular historical circumstances in which he found himself. An intellectual revolution was brought about in Western Christendom by the translation during the 12th and early 13th centuries of the major works of Greek and Arab science and philosophy, and when, in 1266–7, Bacon wrote the *Opus maius* in response to Pope Clement IV's request, his first problem concerned the attitude to be taken to this new knowledge.[2] Following the lead of such men as Robert Grosseteste in Oxford and Albertus Magnus in Paris and Cologne, he tried to show that natural science, far from being a danger to Christendom, was a source of both wisdom and power. The *Opus maius* is in effect a programme of educational reform based on three principal studies—of languages because these were the key to earlier knowledge and to the Bible, in which it was held that all wisdom was contained, at least by implication; of mathematics; and of experimental science. The last two doors to knowledge were the means of investigating nature directly, and it was through his grasp of the possibilities of mathematics as a means of explanation and practical use, and of the role of induction and experimental verification in scientific research, that Roger Bacon made his most important contribution to science. He put forward his ideas on these subjects as an extension of those already advanced by Aristotle in his *Posterior Analytics* and *Physics*.[3]

According to Aristotle, scientific investigation and explanation constituted a twofold process, the first inductive and the second deductive. The

[1] For an account of attitudes to and historical research on Roger Bacon see A. G. Little, 'On Roger Bacon's Life and Works,' in *Roger Bacon Essays*, ed. A. G. Little, Oxford, 1914; 'Roger Bacon,' *Proceedings of the British Academy*, 1928, 14, pp. 265 *et seq.*; J. E. Sandys, 'Roger Bacon in English Literature,' in *Roger Bacon Essays*; Lynn Thorndike, *A History of Magic and Experimental Science*, New York, 1923, vol. 2, pp. 679 et seq.

[2] For Roger Bacon's life see J. H. Bridges, *The Life and Work of Roger Bacon*, London, 1914, and A. G. Little, *op. cit.*

[3] On Aristotle see W. D. Ross, *Aristotle*, 3rd ed., London, 1937, pp. 41–55; R. McKeon, 'Aristotle's Conception of the Development and the Nature of Scientific Method,' *Journal of the History of Ideas*, 1947, 8, pp. 3 et seq.

investigator must begin with what is prior in the order of knowing, that is with facts observed through the senses, and ascend by induction to generalizations or universal forms or causes which are most remote from sensory experience. These universal forms are prior in the order of nature, and the second process in science is to descend again by deduction from them to the observed facts, which are thus explained as following from more general principles which are their cause. The most exact sciences according to Aristotle, and the only ones really deserving the name of science, were those in which the effects could be shown to follow necessarily from their causes as conclusions from premises, and the model of such sciences was mathematics. Where mathematics could be used in the natural sciences the deductions were also exact and necessary, as for instance in astronomy, where the movements of the heavenly bodies could be expressed in geometrical terms, and in optics, where the laws of light were geometrical. These ideas of Aristotle on induction and mathematical demonstration were taught in Oxford by Robert Grosseteste,[1] who said that it was impossible to investigate nature without observation and experiment and to understand it without geometry, and Grosseteste was hailed by Roger Bacon as his master.

Mathematics for Bacon was the 'gateway and key to all other sciences,'[2] the only science capable of giving 'demonstrations of the most convincing kind through a necessary cause.'[3] This he had every reason to believe, because the most mature part of the science that the Latin Christians had learnt from the Greeks and Arabs was mathematical, and in the astronomical system of Ptolemy, the work on optics of Euclid, Ptolemy, and Alhazen, and various works on mechanics based on Euclid and Archimedes, they had examples of exact, demonstrative science. These subjects had excited the greatest interest among Western scientists.

The Ptolemaic system was discussed by nearly every natural philosopher of the 13th century, and it was modified and its range of explanation extended as a result of observations and measurements of the positions of stars and planets with the astrolabe, quadrant, cross-staff, and other astronomical instruments. In optics, the Greek and Arabic writings contained demonstrations of the principles that light travels in straight lines in one medium, and that with reflected light the angles of incidence and reflection are equal; of the phenomenon of refraction (though not the exact mathematical law, which was in fact not formulated till the 17th century); and of many of the properties of plane, convex, and concave mirrors and of lenses. Beginning with this knowledge, Grosseteste tried to improve the theory of refraction and to explain the rainbow, and the Polish physicist Witelo made refraction tables for air, water, and glass. In mechanics, the Dominican mathematician Jordanus Nemorarius and his school demonstrated the law of the lever, and discovered how to determine the component of gravity acting on a body on an inclined plane, an identical proof of which was later given by Stevinus.

This work of his predecessors and contemporaries in mathematical physics Bacon used in Parts IV (Mathematics) and V (Optical Science) of the *Opus maius*, to illustrate the principle that facts observed in the natural sciences are made intelligible, and usable, when they are shown to be the result of mathematical laws; and he tried to extend the application of mathematics to a variety of other problems. He analysed the astronomical grounds for the error in the Julian Calendar, which by the 13th century had become so inaccurate that, as he said, 'any rustic' who looked at the sky could see 'that the new moon occurs as a matter of fact 3 or 4 days before it is marked on the calendar,'[4] and he arrived at a more accurate figure for this error than any previously reached. He tried to calculate the size of the universe, the distances between the planetary spheres, and the dimensions of the sun and moon; to plot the positions of towns and countries on a map; to arrive at the law of freely falling bodies; and to discover the limits of probability in predicting events by astrology, where he held that the planets may influence the human body but not the will, which was free, and that God's omnipotence introduced an element of uncertainty into all judgments about the future.

The chief part of his discussion, however, is taken up with the laws of geometrical optics, and

[1] Mainly in his commentary on the *Posterior Analytics* and his opuscula on optics and astronomy. For the latter see L. Baur, 'Die Philosophischen Werke des Robert Grosseteste, Bischofs von Lincoln,' in *Beiträge zur Geschichte der Philosophie des Mittelalters*, 1912, **9**; and 'Die Philosophie des Robert Grosseteste, Bischofs von Lincoln,' *Beiträge . . .*, 1917, **18**, Hft. 4–6.

[2] *Opus maius*, IV, distinctio i, cap. 1.

[3] *Opus maius*, IV, distinction i, Ch. 3; R. B. Burke, *The Opus maius of Roger Bacon*, Philadelphia, 1928, p. 124. All quotations are from this translation, to which page references are given.

[4] *Opus maius*, IV; Burke, p. 296.

in these he had particular reasons for interest. First, he held that all natural operations in the region below the moon were brought about by the propagation of rays of force, or, as he called it, 'virtue' or 'species,' a theory which in fact went back at least to Lucretius and was very popular among Arab writers.[1] Rays of 'species' or force from the moon, he held, drew up from the sea-floor vapour which pushed up the high tide. Rays from the sun and planets affected the climates of different localities, and rays of astrological influence, acting according to the configuration of the heavens, upset human physiology and affected men's disposition to certain actions; for example, the dreadful comet of July 1264, which was shown to have been generated by Mars, produced an increase of jaundice, leading to bad temper, and the result was the wars and disturbances in England, Spain, and Italy at that time and afterwards! Rays shone forth also from the human soul; Bacon himself had seen a physician blinded by 'species' from the eyes of a diseased patient he was trying to cure; and Antichrist, against whose coming Bacon wished to prepare Christendom, would certainly harm his victims with malignant glances. The type of such a propagation of force or 'species' was light, and with that particular medieval facility for seeing everything implied in everything else, Bacon learnt from St Augustine to see in light the analogy of divine grace and the illumination of the human intellect by divine truth. The study of light was therefore in a special way the key to nature and to truth.

For his basic knowledge of optics Bacon made use of the earlier Greek, Arabic, and Latin Christian writings, and what he added to these was a critical attention to particular problems, with an emphasis on method and on practical applications. He gave a better description of the anatomy of the vertebrate eye and the optic nerves than any previous writer, and recommended that those who wished to study the subject should dissect the eyes of cows or pigs. He pointed out that the sun's rays reaching the earth may be treated as parallel instead of radiating out from a point, thus making possible a better explanation of burning lenses and parabolic mirrors; he discussed in detail the conditions necessary for vision, and working on the theory that the apparent size of an object depends on the angle it subtends at the eye, tried to improve vision by means of lenses. His scientific imagination played freely with the possibilities of indefinitely magnifying small objects and bringing distant objects near, by suitable arrangements of mirrors or lenses. He held that Julius Caesar had erected mirrors in Gaul with which he saw what was going on in England, and that lenses might be used to make the sun and the moon descend and appear above the heads of enemies; the ignorant mob, he said, could not endure it.[2] Spectacles actually came into use a little before the year 1300[3], but the compound microscope and telescope had to wait another three centuries for Galileo and his optic glass.

The mathematical method, as seen in optics, was able to render nature intelligible by making it possible to deduce observed effects from their causes, but neither Aristotle nor any other Greek or Arab writer had given any precise indications of how first to arrive at these causes. This Bacon tried to provide for in his Experimental Science,[4] which forms Part VI of the *Opus maius*, and this, with the previous work of Grosseteste, is possibly the most important contribution to the experimental method between Aristotle or Archimedes and Francis Bacon and Galileo. What Roger Bacon called *Experientia* was both a method and a body of knowledge gained by this method, and its prerogatives were (1) to confirm the conclusions of mathematical reasoning, (2) to add to deductive science knowledge at which it could not itself arrive and (3) to discover departments of knowledge still unborn. As a method of induction, in which he showed how the investigator passes from observed effects to the discovery of the cause and isolates the true cause by eliminating theories contradicted by facts, his procedure is best seen in his attempt to discover the cause of the rainbow. He began by collecting phenomena similar to the rainbow—colours in crystals, in dew on the grass, in spray from mill wheels or oars, or as seen when the sun was looked at through a

[1] On this theory see C. Baeumker, 'Witelo, ein Philosoph und Naturforscher des XIII. Jahrhunderts,' *Beiträge . . .*, 1908, 3, Hft. 2; L. Baur, 'Das Licht in der Naturphilosophie des Robert Grosseteste,' in *Festschrift Georg von Hertling*, Freiburg.-i.-B., 1913; 'Der Einfluss des Robert Grosseteste auf die wissenschaftliche Richtung des Roger Bacon,' in *Roger Bacon Essays*, ed. A. G. Little, Oxford, 1914; L Thorndike, *op. cit.*, vol. 2, pp. 642 et seq.

[2] Donne suggested a similar use for Galileo's telescope in his polemical work, *Ignatius his Conclave*.

[3] See C. Singer, 'Steps leading to the invention of the First Optical Apparatus,' in *Studies in the History and Method of Science*, ed. C. Singer, Oxford, 1921, vol. 2; G. Sarton, *Introduction to the History of Science*, Baltimore, 1931, 2, pp. 1024 et seq.

[4] See on this, R. Carton, 'L'expérience physique chez Roger Bacon,' *Etudes de Philosophie Médiévale*, Paris, 1924, No. 2.

hat or piece of cloth or through the eyelashes. He then examined the rainbow itself, noting that it appeared always in cloud or mist, and by a combination of observation, astronomical theory, and measurements with the astrolabe was able to show that the bow is always opposite the sun; that the centre of the bow, the observer's eye, and the sun are always in a straight line; and that there is a definite connection between the altitudes of the bow and of the sun. To explain these facts he then advanced the theory put forward in Aristotle's *Meteorologica* (III, 2–6), that the rainbow forms the base of a cone of which the apex is at the sun and the axis passes from the sun through the observer's eye to the centre of the bow. The base of the cone would become elevated and depressed, thus producing a larger or smaller rainbow, according to the altitude of the sun, and if it could be sufficiently elevated the whole circle would appear above the horizon, as with rainbows in sprays. This theory he used to explain the height of the bow at different latitudes and different times of year. It would imply among other things that each observer would see a different bow, and this he confirmed by the observation that when he moved towards, away from, or parallel to the rainbow it moved with him relative to trees and houses; a thousand men in a row, he asserted, would see a thousand rainbows, and the shadow of each would bisect the arc of his bow. The colours and form of the rainbow, he held, therefore bore to the observer a relation unlike those of fixed objects such as crystals. About the colours, Bacon's discussion was as unconvincing as that of everyone else till Newton; the form he explained as due to reflection from spherical water drops in the cloud, the rainbow of any particular observer appearing only in the drops from which the reflected rays went to his own eyes. This theory he extended to try to explain halos and mock suns. It was not in fact correct, and the correct explanation was given a little later by a younger contemporary of Bacon's, Theodoric of Freiberg (d. 1311) (following Al-Farisi),[1] and again more fully by Descartes in his *Dioptrique*.

Bacon's ideas on the mathematical and experimental methods had an immediate and lasting influence on his successors.[2] He seems to have inspired the study of optics and physics in Oxford, and of astronomy in Paris, at the end of the 13th century. He was famous throughout the later middle ages as an alchemist, and his medical writings were collected in the 14th century and were still being used by physicians in the 17th. His idea that there was no great width of ocean between Spain and India indirectly influenced Columbus; his work on the correction of the calendar seems to have led to the suggestion that Copernicus should undertake the same task; and the section of the *Opus maius* on Optics was continuously copied, was printed as late as 1614, and must have been known to Kepler and Galileo. So his prophetic imagination has at least some strands of connection with real history.

But later ages gradually forgot the primary purpose of Roger Bacon's suggested reforms and what they meant to himself, and created the legend of him as the prophet of pure utilitarianism. So to conceive of him would be to contradict everything we know about his age and entirely to miss the point of the *Opus maius*. When

> 'science held her seate
> Between the circled arches of (his) browes,'

it was not simply for acting 'strange and uncoth' miracles; and in the last part of the *Opus maius*, which is concerned with what he called Moral Science, he gathered up the other parts into their ultimate function, namely to protect Christendom and to assist the Church in her work of evangelizing mankind by leading the mind through scientific truth to the contemplation of the Creator already revealed in theology, a contemplation in which all truth was one. This was the real purpose of power and wisdom and the ultimate utility.

[1] E. Krebs, 'Meister Dietrich (Theodoricus teutonicus de Vriberg), sein Leben, seine Werke, seine Wissenschaft,' *Beiträge . . .*, 1905, **5**, Hft. 5–6; J. Würschmidt, 'Dietrich von Freiberg über den Regenbogen und die durch Strahlen erzeugten Eindrücke,' *Beiträge . . .*, 1914, **12**, Hft. 5–6; cf. P. Duhem, *Etudes sur Léonard de Vinci*, Paris, 1906, vol. 1, pp. 171 et seq.

[2] On Roger Bacon's influence see J. H. Bridges, *op. cit.*; A. G. Little in *Roger Bacon Essays*, pp. 24 et seq.; P. Duhem, *Le Système du Monde*, vol. 3, pp. 446 et seq., 511 et seq.; D. E. Sharpe, *op. cit.*, pp. 117–20; L. Thorndike, *op. cit.*, vol. 2, pp. 616 et seq.

Imitating the clouds

M. W. CHIPLONKAR

A good method of learning is to imitate, and the history of science abounds in pioneer experiments deliberately arranged to imitate Nature. An attempt has been made in this article to show how imitation has helped us to get an insight into the various physical processes going on in the atmosphere, which often produce most beautiful and arresting clouds.

To the scientific mind clouds present an almost unlimited field of inquiry, and it is an absorbing problem to try to understand and imitate their shapes. It is clear that to attempt to solve the problem purely mathematically is an utterly hopeless task, and one has to fall back on some sort of experimental method. Here too the challenge of only the simpler and not the complicated problems has to be taken up if one wishes to achieve anything like success. The more regular and uniform patterns are obviously the most likely to be due to simple physical conditions existing at the time in the atmospheric layers in which they are produced. In fact, it is these simple regular patterns of natural clouds that have been well imitated in the laboratory in recent times.

At first, Helmholtz and Lord Kelvin tried to explain theoretically the processes underlying the formation of such cloud patterns in the sky, taking for their basis the relative movements of the neighbouring air layers; they were, however, not successful. H. Benard, and later P. Idrac and T. Terada, suggested from their laboratory experiments that the patterns might be due to the same process as that which gives rise to the convection cells (now known as Benard cells, figure 1) in a shallow unstable layer of a fluid. It is also reported that this kind of regular organized movement was observed much earlier by Weber, in very thin films of liquid mixtures on a microscope slide, and also by James Thomson in deeper layers of soapy solutions left undisturbed in an open tub. The experimental work of later investigators on instability layers of fluids, with or without a superposed general shear, has not only supported their suggestion but has further elucidated many other points of detail demanded by the theory developed first by Lord Rayleigh and later modified by Harold Jeffreys.

In the course of his studies on clouds, natural and artificial, the author was fortunate enough to secure a number of typical photographs of natural cloud patterns as they developed and dwindled away. An attempt is made in this article to present, out of that collection, a few typical natural cloud patterns side by side with their corresponding laboratory imitations,[1] and to discuss each briefly in the light of the above theory.

It is not very difficult to produce these cloud patterns in a laboratory. The photographs reproduced here were, however, obtained by employing specially constructed apparatus. A uniformly heated metal plate formed the bottom of a shallow chamber, and a long thick glass plate which could be moved in the direction of its length served as its top. The thickness of the air layer enclosed between the two plates (i.e. the height of the chamber) could be varied from a few millimetres to a few centimetres, by using accurate distance pieces. Heating at the bottom (or cooling at the top, or both) produced instability in the layer, and steadily moving the glass plate produced a uniform shearing motion in it. Tobacco smoke was introduced into it through a side tube to make visible the patterns formed in the air layer, which were then photographed from above under various known conditions.

For the purpose of this discussion the patterns are divided into the following three types:

I. Those showing irregular polygonal cells.
II. Those showing long rolls or waves with or without polygonal cells.
III. Those showing polygonal cells arranged in rows.

I. IRREGULAR POLYGONAL CELLS

Figure 2(*a*) is a photograph of an overhead strato-cumulus cloud taken at 11 a.m. on 10th April, 1939. There was no movement of the cloud as a whole in any direction, but the sheet of uniform cloud originally formed as a thin layer

[1] The author is greatly indebted to Mr K. Chandra for his laboratory imitations of cloud patterns reproduced in this article.

was seen to break up into numerous irregularly set polygonal cells (shown separately in the trace in figure 2(*b*)), and at the same time to grow thicker. The internal movements in this cloud did not produce any great changes in the pattern except the strengthening or weakening of some of the lines and foci of descent. The cloud once formed was quite stable, and actually remained unchanged for more than an hour. An imitation pattern of the above is shown in figure 2(*c*), which is a photograph of an unstable layer of air mixed with smoke to make the movement visible. No bodily movement existed in this experimental layer either, but the dark lines and centres were seen to appear and disappear slowly. The resemblance between figure 2(*a*) and figure 2(*c*) is remarkable.

Figure 3(*a*) is a photograph of an overhead alto-cumulus cloud taken on a fairly clear day in October 1940. In this, the polygonal cells are much smaller in size and are more clearly defined. Here also very little, if any, movement of the cloud as a whole existed, but the cloud itself soon disappeared. A few of the polygonal cells are shown separately in figure 3(*b*), by tracing their boundaries. Figure 3(*c*) shows the corresponding type of cellular structure observed experimentally in a shallow layer (7 mm depth) with a large difference of temperature (130° C) between the top and the bottom of the layer. Here also the likeness between the natural and the artificial patterns is striking.

The movement of fluid in such a convection cell, the normal Benard cell (see figure 1), is that of a vertical ascent in the centre, a flow outwards from the centre in the uppermost layer, a vertical descent all along the periphery, and finally a flow inwards toward the centre in the lowest layer of the cell. In the sky, the white cloud material which may exist already, or which may be formed by fresh condensation above the ascending currents, is collected together at such places as C, C, while at A is formed the dark (blue sky) centre, and at places such as B, B is formed the dark (blue sky) boundary of the cells. When the supply of cloud material is abundant, the portions at C, C join up and the dark centre is not apparent. A circulation in the reverse direction is also possible, and is in fact often noticed in nature; the ascending current is along the periphery and the descending one at the centre of the cell. Such a process will obviously produce a pattern complementary to that of the normal Benard cell, i.e. a network of filaments of the white cloud material with empty circular blue patches of sky in the centre.

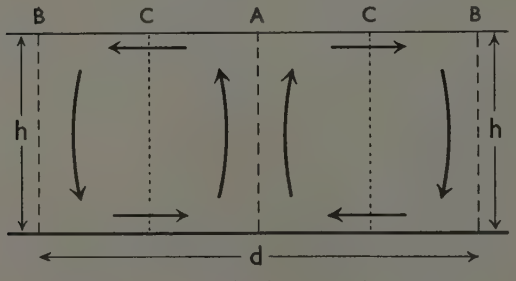


FIGURE 1 – *Circulation in a normal Benard cell.*

A very simple but interesting point may be raised here regarding the two motions. Are they not one and the same thing, except for a change of sign, the normal type of motion being as shown in figure 1 and the reverse motion as that shown when the same diagram is inverted or viewed upside down? Mathematically, the two motions represent the two solutions to the same problem of an unstable layer of fluid, and are identical except for a change of sign. Both solutions must be equally applicable, but in actual practice when working with a liquid it is found that the liquid always rises in the centre of the cell (normal type) whether we heat it at the bottom or cool it at the top; with air it always descends in the centre (reverse type). It has been suggested, after careful investigation, that the real explanation of this difference in motion is to be found in distinguishing the initial type of motion from the final steady viscous motion. The significant point to note here is that both types of motion actually occur in the atmosphere, and produce two distinct varieties of cloud patterns, depending on the prevalent conditions of temperature-lapse rate, humidity, etc., in the air layer. Figure 4(*a*), and its tracing in figure 4(*b*), provide an example of such a reticular pattern in nature, and figure 4(*c*) is its imitation in the laboratory. It presents the queer appearance of a sheet of white paper punched irregularly with circular holes. In this connection it may be pointed out that in the atmosphere, as it is actually constituted, more air goes up by chimney action than comes down; and it is this property of the atmosphere that, perhaps, favours the normal Benard cell or the chimney circulation more than the reverse one.

II. LONG ROLLS AND WAVES WITH OR WITHOUT POLYGONAL CELLS

Coming to the second group of patterns we see

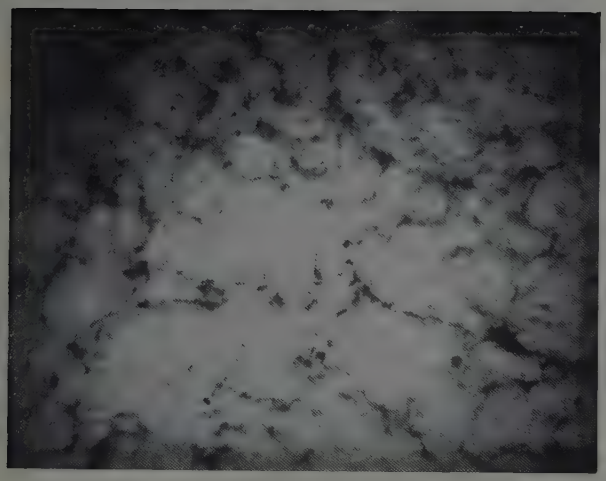


FIGURE 2(*a*)

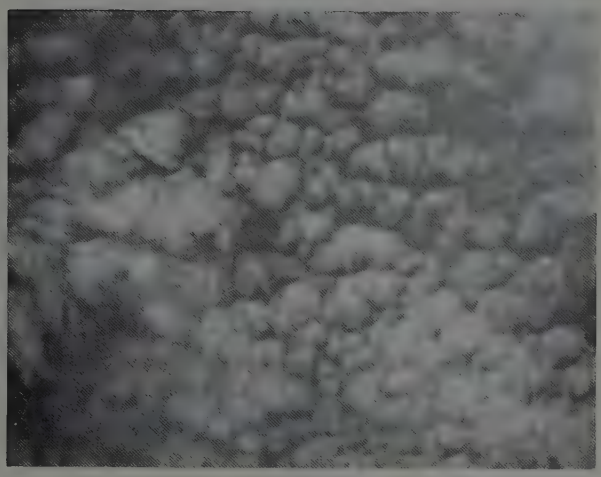


FIGURE 3(*a*)

FIGURE 2(*b*)

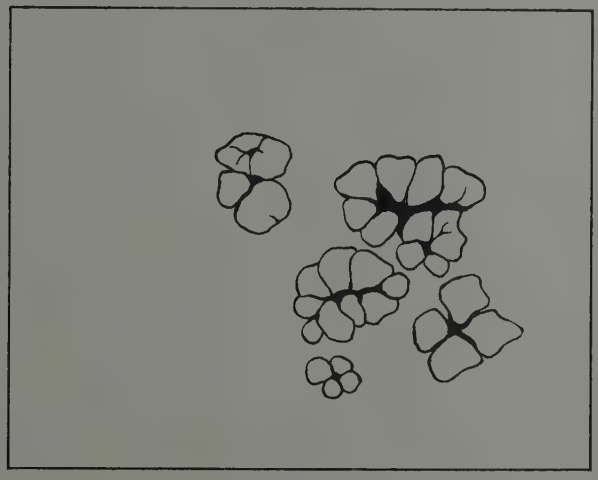


FIGURE 3(*b*)

FIGURE 2(*c*)

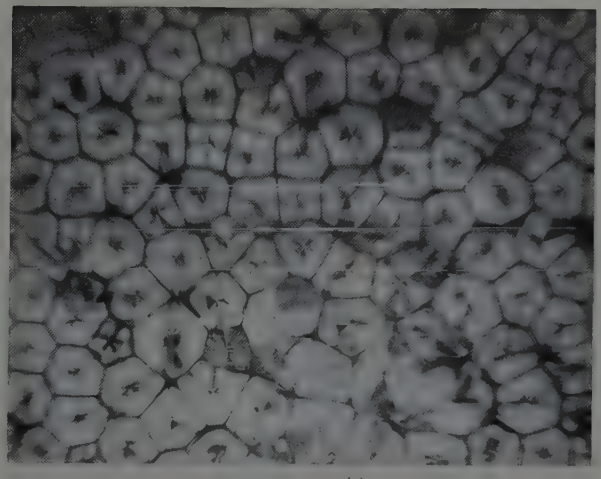


FIGURE 3(*c*)

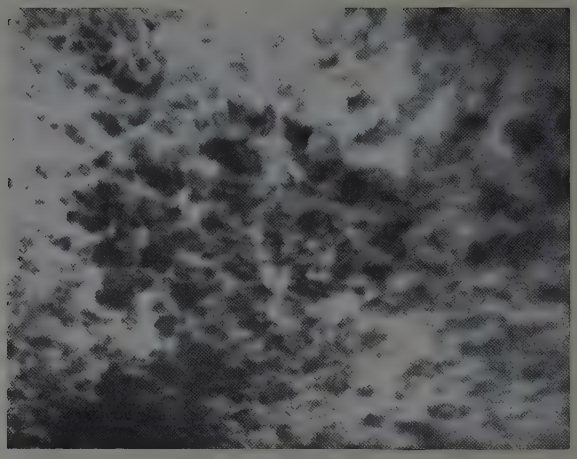


FIGURE 4(*a*)

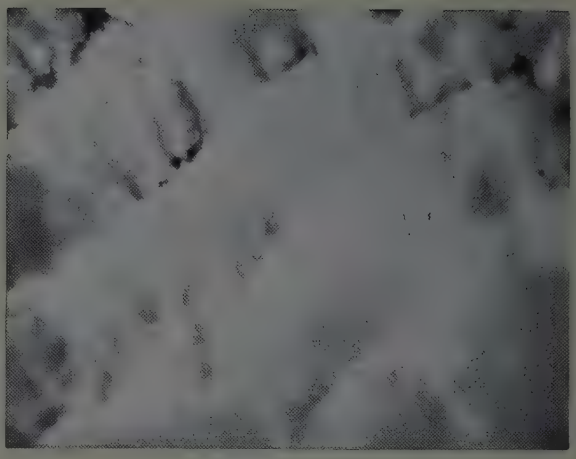


FIGURE 5(*a*)

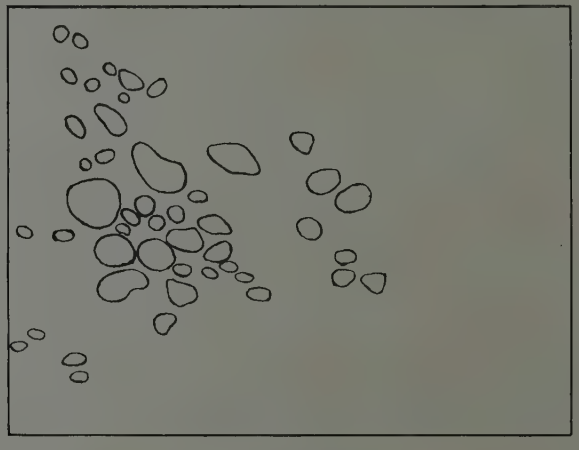


FIGURE 4(*b*)

FIGURE 5(*b*)

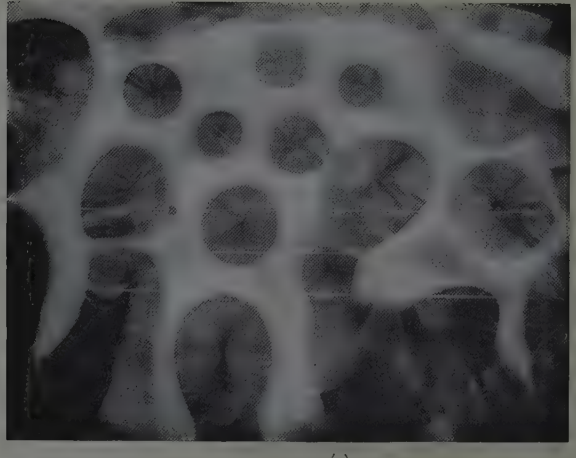


FIGURE 4(*c*)

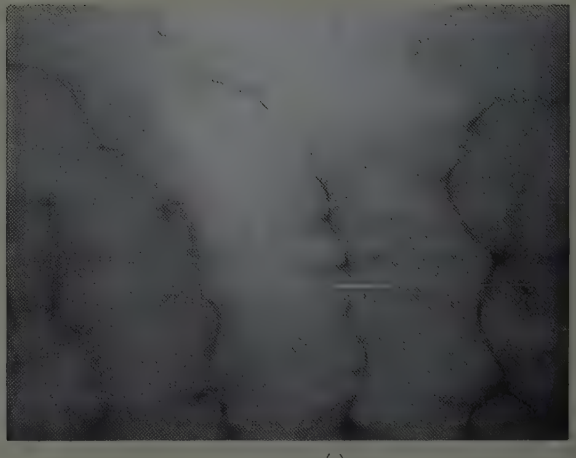


FIGURE 5(*c*)

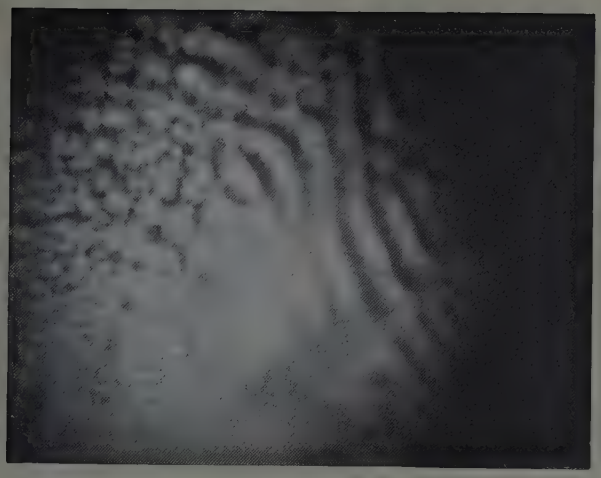


FIGURE 6(*a*)

FIGURE 7(*a*)

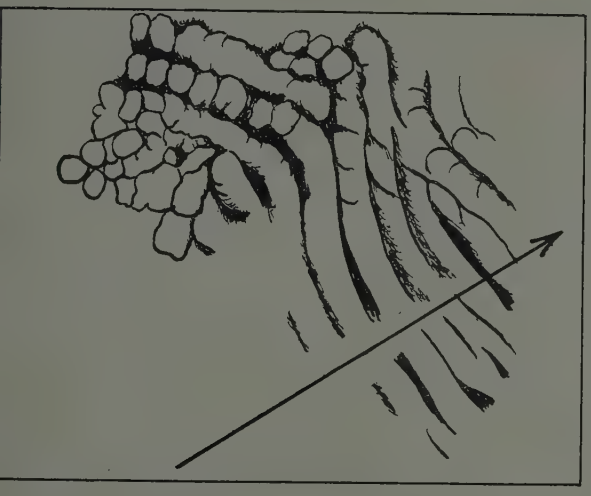


FIGURE 6(*b*)

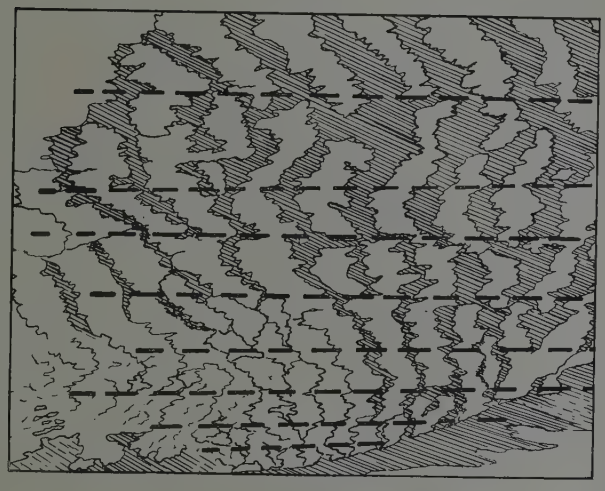


FIGURE 7(*b*)

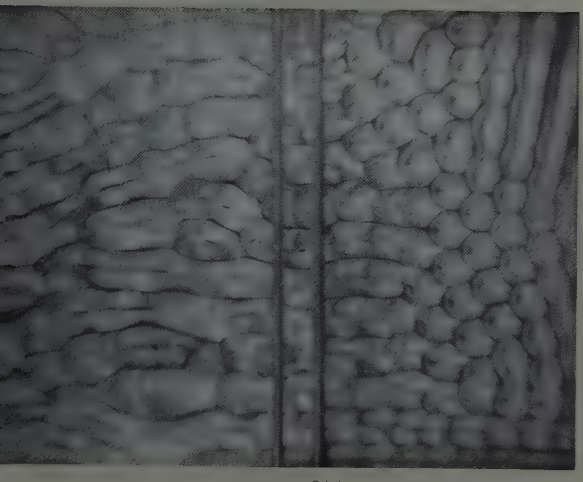


FIGURE 6(*c*)

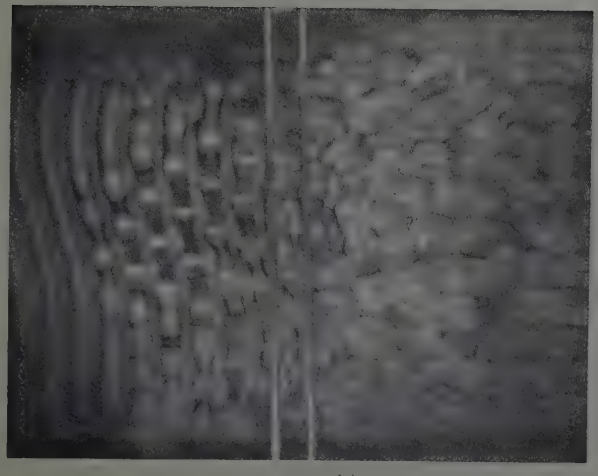


FIGURE 7(*c*)

Figure 7 (c) is reproduced by permission of the Royal Society.

FIGURE 8(*a*)

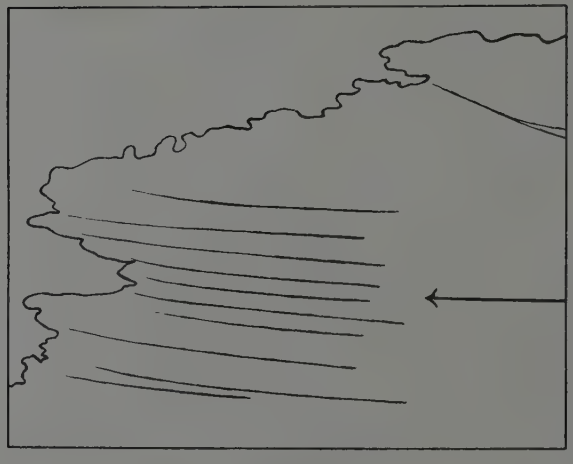


FIGURE 8(*b*)

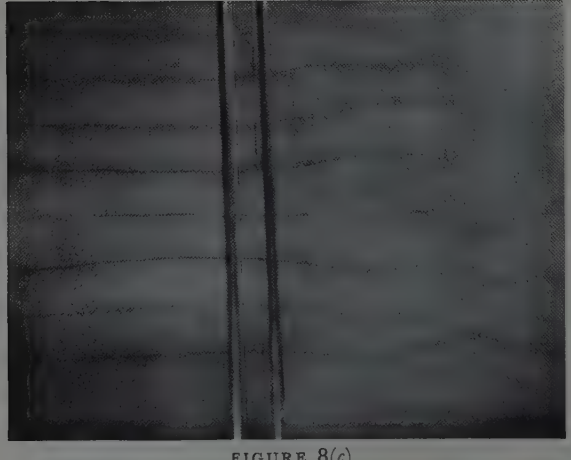


FIGURE 8(*c*)

that it includes two distinct types of roll structure: (1) transverse wavy rolls, and (2) longitudinal straight rolls.

1. *Transverse Rolls*

Figure 5(*a*) is a photograph of a very low alto-cumulus cloud with thick rolls taken on 12th April, 1939, with the camera pointing to the zenith. The cloud bank was observed to move as a whole in the direction shown by the arrow in the tracing in figure 5(*b*), with the rolls lying at right angles to it. The front sides of the rolls are very sharply defined, and each roll shows in itself a less defined structure of polygonal or round cells. The resemblance to the laboratory pattern, figure 5(*c*), which was developed in a 12 mm layer of air, with a temperature difference of about 95° C between the top and the bottom, and a small shear movement from the left to the right, is remarkably perfect. An extremely beautiful photograph of a wavy transverse pattern of an alto-cumulus cloud is shown in figure 6(*a*). In almost every cloud of this kind any small obstacle, or a sudden discontinuity in the physical conditions of the atmospheric layer, causes a break in the pattern, and it is continued from roll to roll in the direction of the shear. It will be noticed in the tracing, figure 6(*b*), that the various points of break in the different rolls lie on straight lines parallel to the direction of shear. This may also be regarded as a tendency to change over to a cross pattern, or to longitudinal rolls in the same sheet of cloud, as described later.

An interesting example of transverse rolls and polygonal cells in the same sheet of alto-cumulus cloud is shown in figure 7(*a*). This is one of a series of photographs taken on the morning of 19th July, 1939, with the camera almost facing the zenith. The sequence of movements showed that the cloud material of the freshly formed cellular pattern was gradually being moulded into the transverse rolls, owing to a small shearing motion at the cloud level. The direction of motion in the cloud marked by the lines in figure 7(*b*) is more or less normal to the length of the rolls produced. A very similar laboratory pattern was obtained under similar conditions, viz. the superposition of a small amount of shear on an already existing polygonal structure (see figure 7(*c*)). Actually the depth of layer was 8 mm, the difference of temperature between the top and the bottom was 90° C, and the shear was equal to 1·7 cm/sec approximately.

2. *Longitudinal Rolls*

The only convincing photograph of straight longitudinal roll cloud that the author could get is reproduced in figure 8(*a*). It is one of a series of photographs of the spread-out portion of the large anvil of a cumulo-nimbus cloud. It was taken late in the evening of 30th March, 1939. The rolls marked by the flow lines in the trace in figure 8(*b*) were actually seen to develop when the cloud was overhead, but at that time the lower scuds did not continuously allow a complete view of these rolls. The series of photographs could therefore be taken only after about 40 minutes, when the low clouds moved away. The whole anvil in the meantime had moved sideways, and presented a more or less profile view. When overhead (and to a less degree in the position shown in figure 8(*a*)) the rolls had a very close resemblance to the pattern in figure 8 (*c*), obtained in the laboratory while working with a layer of 16 mm depth, a small difference of temperature (about 30° C), and a large amount of shear (about 10 cm/sec).

It is occasionally observed that such rolls have wavy boundaries and are also bent at the ends (see figures 9(*a*) and 9(*b*)). They are mostly due to the original high shear falling at a later stage below a certain value, while the convection caused by the thermal instability still retains its original strength. The corresponding laboratory pattern shown in figure 9(*c*) was obtained while working with a layer of 16 mm depth, a large difference of temperature, and a comparatively small amount of shear, varying from 5 to 8 cm/sec.

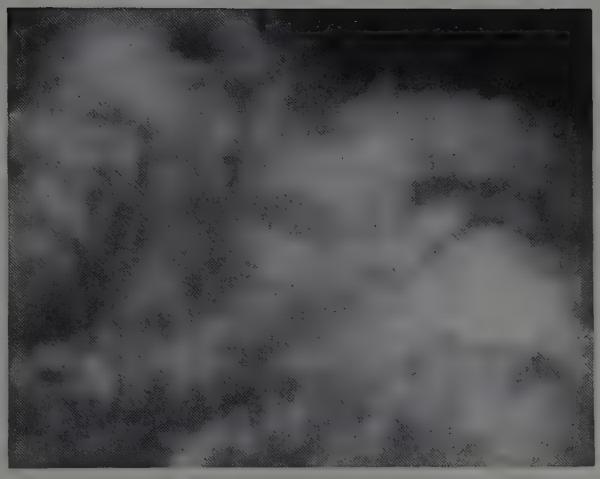


FIGURE 9(*a*)

FIGURE 9(*b*)

III. POLYGONAL CELLS ARRANGED IN ROWS

In this group all the cloudlets are arranged in two sets of parallel rows, and the shape of each cloudlet is determined by the angle between the rows. They can thus be rectangular or parallelogrammic.

An excellent example of such a pattern with rectangular cloudlets is shown in figure 10(*a*), and its tracing in figure 10(*b*). This pattern of alto-cumulus was photographed on 18th July, 1939, with the camera facing west at an angle of 50° above the horizon. The cloud was gradually evaporating, as is well seen in the series of pictures of this cloud. The pattern has been shown to be produced in the laboratory when an intermediate amount of shear is superposed on the thermal instability. Such conditions, being very critical, are somewhat difficult to attain. The cloudlets then arrange themselves either parallel and perpendicular to the direction of shear, or

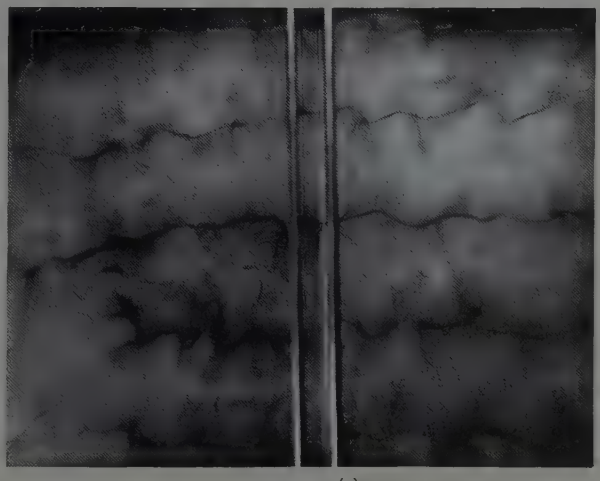


FIGURE 9(*c*)

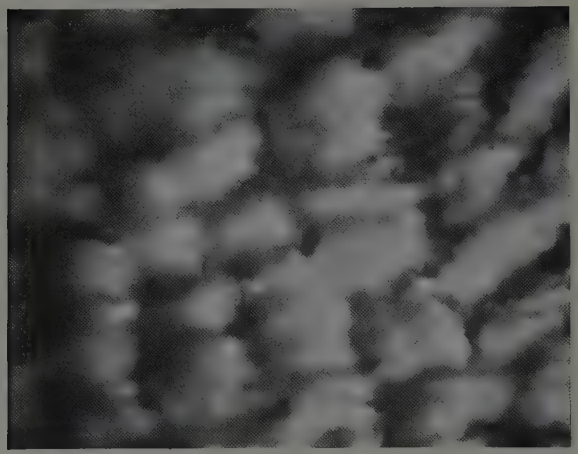


FIGURE 10(*a*)

FIGURE 11(*a*)

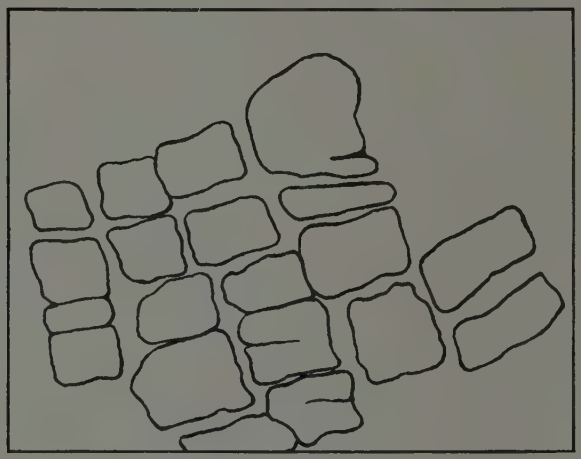


FIGURE 10(*b*)

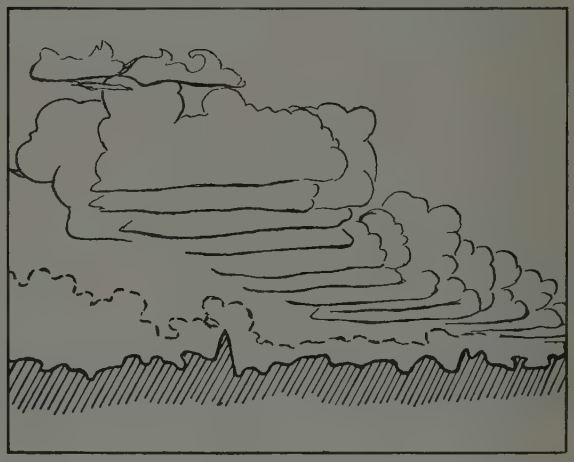


FIGURE 11(*b*)

FIGURE 10(*c*)

at an angle of 45° to it. The laboratory imitation, which compares very well with this, is shown in figure 10(*c*), which is reproduced here from Sir Gilbert Walker's article in *Science Progress* (1935).

The cellular pattern associated with larger cumulus clouds has been found to be of great practical utility in modern times, and has attracted the attention of the aviator. A typical example of such a pattern is shown in figure 11(*a*) and its tracing in figure 11(*b*). Because of the perspective appearance it is sometimes called an echelon cloud. To the pilots of glider planes it is known as 'the cloud street.' Advantage is taken of the ascending currents of air under each cumulus to gain height, and the glider goes hopping from one cloud to another, i.e. from one centre of the convection cells to another.

What is a glass?

G. O. JONES and F. E. SIMON

From the standpoint of formal thermodynamics, none of the terms 'solid,' 'liquid,' 'supercooled liquid' completely defines the condition and properties of a substance in the glassy state. A glass is in fact a non-thermodynamic phase. Dr Jones and Professor Simon show that an understanding of the implications of this fact is essential both for the explanation of the unique properties of glasses and for an understanding of the Third Law of Thermodynamics.

The most characteristic property of a glass, and that which makes possible the familiar techniques of glass-blowing, moulding, and drawing, is that when it is heated it softens progressively. While the melting of a crystalline solid into a liquid is always accompanied by a sudden fall in the resistance to deformation, and by changes in all other physical properties, we may say that a glass loses its solid-like character gradually, by means of a continuous decrease in the value of the viscosity. Thus at room temperature the viscosity of a window-glass is so high that viscous flow is almost impossible to detect, and the most obvious quality of the material is its elasticity or rigidity. At higher temperatures, however, viscous flow becomes more and more apparent under the smallest stresses. As a matter of fact there is still elastic resistance to all types of deformation even at high temperatures, but this is elusive on account of the low viscosity.

A graphic illustration of the way in which the behaviour of glass changes continuously as it is heated is afforded by considering the properties of elasticity and flow. The curves of figure 1 show how the deformation (or strain) increases with time under, say, a constant shear stress at various temperatures [1]. At low temperatures, the behaviour under constant stress is just what we should expect from a solid, that is, the glass shows an immediate deformation, which then remains practically constant and is actually recovered on removal of the stress. At the higher temperatures represented, the most obvious property is the viscous flow, but the recoverable elastic deformation can still be detected. There is, however, an enormous decrease in the value of the viscosity as the temperature is increased, which changes the whole character of the curves. Thus the slight increase in free volume gives the atoms or molecules much greater freedom to diffuse or to change places, but without greatly affecting the average interaction between them.

The progressive softening or hardening has a more than purely practical importance: it is also the feature of greatest scientific interest. X-ray pictures show that the structures of glasses are quite similar to those of ordinary liquids, and we are thus enabled to study typical 'liquid' properties with relatively simple experimental techniques, since we have, as it were, a slow-motion model available.[1] Also, as we shall see later, we can isolate, or focus on, certain important properties in a way which is otherwise impossible, and

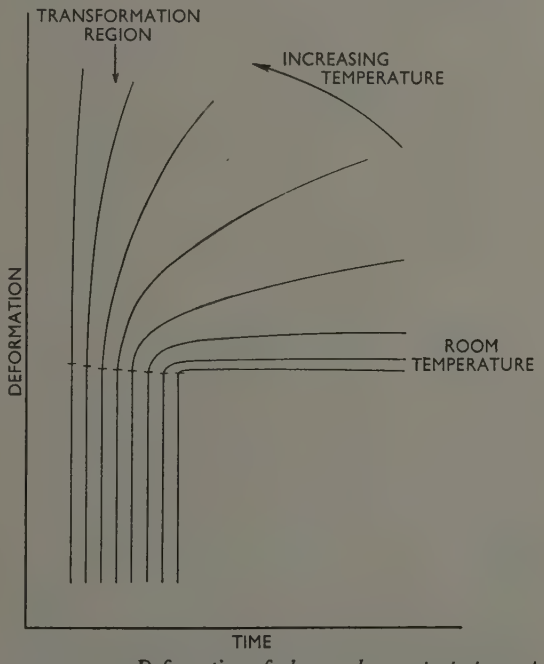


FIGURE 1 – *Deformation of glass under constant stress at differing temperatures.*

[1] For example, we may guess from figure 1 that the flow behaviour of a liquid probably does not consist only of Hookeian elasticity plus Newtonian viscosity, as assumed by Maxwell in his well-known equation of 'visco-elasticity.'

the results of these studies are of great theoretical interest. And, of course, the properties of ordinary glasses at room temperature—not all of them desirable—are determined by the kinetics of this hardening process.

THE THERMODYNAMICS OF GLASS

Let us first consider how glass is to be classified from a thermodynamic point of view. We shall for the present understand by the term 'glass' any substance formed by continuous cooling from the liquid condition such that its viscosity has risen to about 10^{13} poises or greater. The significance of this figure will be discussed later. (The viscosity of ordinary glasses at room temperature is actually much higher than 10^{13} poises, and they behave as brittle solids.) In the typical case this means that we have a liquid super-cooled below its freezing point and therefore not thermodynamically stable.

We might expect that the tendency to crystallize or devitrify—that is, to reach the state of lowest free energy—would be the most important problem in all studies on glass. This is not so, however. As shown by Tammann [2] and his school, the tendency to devitrify does not increase continuously as the glass is cooled below the liquidus temperature, but in the absence of artificial nuclei there is a fairly narrowly defined 'dangerous range' of temperature for devitrification, well below the liquidus temperature. (The viscosity at the liquidus temperature is usually about 10 poises, and at the dangerous range for devitrification about 10^4 poises.) It has generally been considered that this is because the two main factors affecting the rate of devitrification lead to effects which vary with temperature in opposite ways. Thus the free energy difference between solid and supercooled liquid increases as the temperature is lowered, but the viscosity, which resists the process, also increases rapidly. The theory has been developed and refined by Becker [3], Frenkel [4], and others to take account of the question of the stability of embryonic nuclei formed by thermal fluctuations—or self-nucleation. In all inorganic glasses which are of practical use and commercially made we are always well below this dangerous region, so that devitrification is of negligible importance and we need not consider it further.

The ability of a substance to super-cool and finally to form a rigid glass is determined by its structure in the liquid condition, and can very roughly be related to the viscosity, or to the activation energy constant (B) in the well-known relation ($\eta = Ae^{B/RT}$) between viscosity and temperature, derived by Andrade [5] and others. A high value of this activation energy means that the atoms or molecules cannot easily change their relative positions, and this is obviously some measure of the resistance to crystallization; it also means a high rate of increase of viscosity with falling temperature and thus a good probability of forming a rigid glass at some lower temperature.

Is glass then nothing more than a super-cooled liquid ? The answer to this is 'No.' At sufficiently low temperatures all glasses show phenomena of a new and unusual kind, and we must consider these in detail because of their thermodynamic—and practical—significance. As was shown by Tammann [6], all glasses, super-cooled liquids, or other amorphous substances show rapid changes, at particular temperatures, in such properties as the coefficient of thermal expansion, and the value of the viscosity at this temperature is always found to be about 10^{13} poises. For a long time the significance of this clue was missed in spite of the fact that these anomalies occurred in such widely differing substances as glycerol (at about −100°), selenium (at room temperature), ordinary inorganic glasses (at 400°–600°), and fused silica (at about 1200°), and the suggestion was frequently advanced that glass below this transformation temperature, as it is usually called, was a 'fourth state of aggregation.'

The explanation was given by one of us [7] about 1930, following a series of experiments [8] concerning the general validity of the Third Law of Thermodynamics (Nernst's Heat Theorem). This law postulates that entropy differences between all states of a system disappear at absolute zero. Now since entropy is a measure of the degree of order, this would mean that at absolute zero a crystal and a glass should be in the same state of order. However, we know from X-ray studies that the atoms in an ideal crystal are arranged in the perfect order of a lattice, whereas the arrangement of the atoms in liquids or glasses has only a degree of short-range order.

Thus one could not see how the Third Law could be obeyed by such a system. In order to find out more about this point, the specific heat of glycerol was measured down to liquid-hydrogen temperatures (a little later the same substance was also investigated by Gibson and Giauque [9]). The results are shown in figure 2. We see that there is no discontinuity in the specific heat as we pass from stable liquid above the melting point to super-cooled liquid below, the value thus still being very

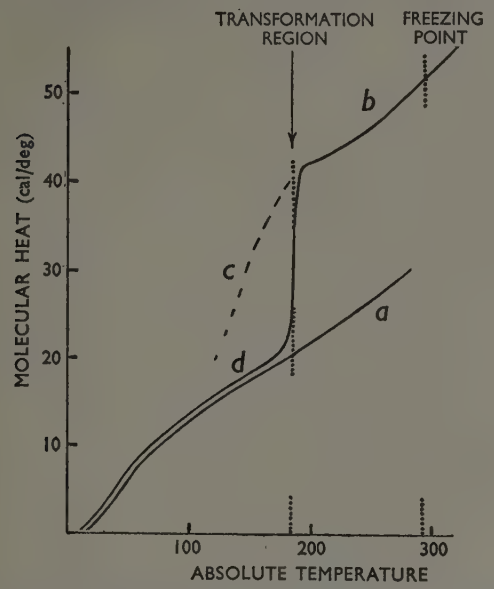


FIGURE 2 – *Molecular heat* (C_p) *of glycerol.* (a) *Crystal,* (b–d) *liquid, super-cooled liquid, and glass,* (c) *expected form of equilibrium curve for super-cooled liquid.*

much higher than that of the crystal. This state of affairs continues until about −100°, when a relatively sudden fall occurs, nearly to the value shown by the crystal. The region of the anomaly is the transformation temperature or, preferably, the transformation region.

From the data of these curves and the known heat of melting, it is possible to calculate the entropies of the two phases, and the entropy differences are shown in figure 3. It is obvious that the entropy difference between the liquid and

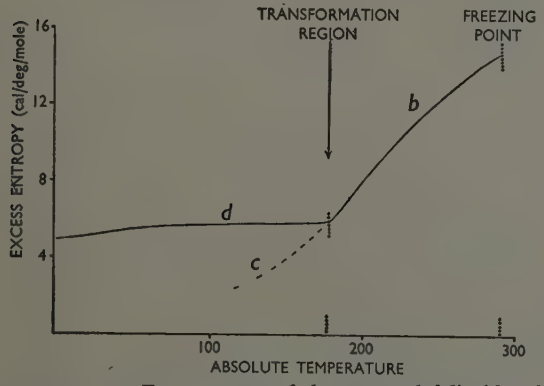


FIGURE 3 – *Excess entropy of the super-cooled liquid and glass over the crystal (glycerol).* (b) *Super-cooled liquid,* (c) *expected form of equilibrium curve for super-cooled liquid,* (d) *glass.*

the crystal does not drop to zero but remains nearly constant below the transformation temperature. Thus we seem to have a *prima facie* violation of the Third Law.

Does this behaviour actually constitute a violation of the law? The answer is given by the analysis of the specific heat curve. What is the significance of the high specific heat of the amorphous phase above the transformation region? We know that the viscosity is about 10^{13} poises at the transformation temperature. This corresponds to a Maxwell relaxation time[1] of the order of seconds or minutes, and suggests that the effect is in some way connected with atomic or molecular motions which are damped or restricted by neighbouring molecules.

One might thus imagine that the anomaly in the specific heat was due to the possibility of new degrees of freedom, such as would come into play if torsional vibrations could change into rotations above the transformation region. It can easily be shown, however, that not more than about a quarter of the total magnitude of the anomaly could be explained in this way.

The explanation is quite different. The entropy, or disorder, of a substance is made up of a number of contributions. First, there is the thermal motion (translational, vibrational, and rotational) in all states of aggregation. This is usually the only contribution in a gas, and also in a crystal, where the centres of the molecules are more or less fixed in lattice positions. In a liquid, however, things are different. Although at one time liquids were generally regarded as systems of complete disorder, we now know that they have a degree of short-range configuration order. The forces of attraction between atoms are order-producing, and also, as has been illustrated by Hildebrand [10] on the basis of a 'tennis-ball' model, the mere fact that atoms retain their impenetrability or mutual repulsion is enough to lead to a certain degree of order.

Now, disorder increases with temperature, and this fact contains the explanation of our specific heat anomaly. The change of configuration with temperature must have an effect on the specific heat, since we have to supply energy to increase the disorder, and it is actually mainly responsible for the fact that the specific heats of liquids are higher near their melting points than those of their corresponding solids. This contribution is

[1] Defined as the time in which the stress required to maintain a constant strain falls to $1/e$ of its original value through viscous relaxation.

not easily detected, because only relatively complicated liquids super-cool, and the values of their specific heats are too difficult to interpret. However, the fact that in the transformation region the mobility of the molecules disappears more or less sharply causes this contribution to the specific heat to drop out, and in doing so to reveal itself strikingly.[1] It is clear that this configurational specific heat, as it has been called by Bernal [11], is of essential significance, and no theory of the liquid state can be complete which does not account for it. Bernal's molecular theory of liquids, which is based on a number of simple assumptions and uses a semi-empirical model of liquid structure, contains a representation of this quantity, and he also shows how the equilibrium degree of order or configuration can be a function of temperature and pressure.

If this explanation is correct, the specific heat below the transformation region must be a function of time, and if sufficient time could be allowed for internal equilibrium to be continuously established when measuring the specific heat, it should follow a continuation of line (*b*), as indicated by the dotted line (*c*) in figure 2. To test this prediction, Oblad and Newton [12] carried out specific heat measurements on glycerol, in which times up to one week were allowed for the establishment of equilibrium.[2] Their values agreed well with those predicted, and they were able to follow the equilibrium curve to about 15° below the transformation region. The exact location of the transformation region thus depends on the time-scale of the experiments, and it appears at the temperature at which the equilibrium time of configurational adjustments crosses this time-scale.

By allowing still longer times for adjustment it should be possible to follow the dotted line to still lower temperatures and thus to still smaller entropy differences. Although a practical limit is very soon reached, it is safe to predict that, by an infinitely slow approach to the absolute zero, one would approach vanishing entropy differences.[3]

Thus the picture which we have is the following. In substances which can be super-cooled considerably, the properties of the super-cooled liquid are at first continuous with those of the stable liquid, the system being in *internal* equilibrium. In this condition the substance rests in a free-energy minimum, but not in the lowest possible minimum—that of the crystal. The mutual positions of the molecules are those of thermal equilibrium, and the state of the substance is completely described if we know the number and kind of particles and the temperature and pressure. Below the transformation region, however, the system loses its ability to readjust its configuration. It is for this state that we reserve the term 'glass' (a definition which accords well with the popular use of the term, since the great practical usefulness of glass arises from the fact that configurational changes in the material are extremely slow). In a glass, changes of temperature result, to a first approximation at least, only in changes in the thermal vibrations, which are more or less the same as in the crystal—the specific heat is actually slightly larger owing to the larger volume of the glass. A glass is not in internal thermal equilibrium. The system cannot be defined by stating the number and kind of particles and the pressure and temperature. We have already seen that slow cooling leads to

[1] It is important to realize that the relatively sudden drop in specific heat in the transformation region is in no way analogous or similar to the so-called 'lambda' phenomenon in the specific heat curves of substances which show order-disorder transitions. In our case the drop is due to the fact that internal equilibrium is *not* reached below the transformation region, while at all points on the lambda curve the substance is in internal equilibrium and the relatively sharp change is due to the development of a particular kind of co-operative phenomenon. Of course the *total* configurational entropy is the expression of a partial order-disorder transition, namely that between the state of order of the liquid at the absolute zero and its state of order at the melting temperature.

[2] An elegant experimental proof of the reality of the equilibrium in the super-cooled liquid has been given by Lillie [13]. By carrying out appropriate heat-treatments he was able to establish configurational temperatures both above or below a chosen temperature. When the temperature was then rapidly adjusted to this chosen value he was able to show that the viscosity (as measured) could approach its equilibrium value from either side.

[3] It can easily be shown from the thermal data already mentioned that the internal energy difference between the super-cooled liquid and the crystal will not vanish, and it is perhaps worth emphasizing that the 'equilibrium' glass formed by infinitely slow cooling will not become identical with the crystal. If we want to speculate about the configuration of this glass at the absolute zero, it may be useful to remember that one liquid actually exists for which an approach to the absolute zero can be realized experimentally, namely helium. Owing to the high zero-point energy of helium this liquid is more stable under its own vapour pressure than is the solid. Liquid helium, indeed, approaches the same entropy at absolute zero as solid helium—which is stable above 25 atmospheres pressure—in spite of a quite different arrangement of the atoms. In this particular case at least the loss of entropy is not due to the establishment of geometrical order, but order is set up in 'momentum space.'

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E. ASHBY, D.Sc., A.R.C.S., F.L.S., has been Professor of Botany and Director of the Laboratories in the University of Manchester since 1947. During the war he acted first as the Director of the Commonwealth Scientific Liaison Bureau, and subsequently as Counsellor in the Australian Legation at Moscow.

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HARRIETTE CHICK, D.B.E., D.Sc., Hon. D.Sc., is a Fellow of University College, London, and a member of the staff of the Lister Institute of Preventive Medicine.

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M. W. CHIPLONKAR, M.Sc., D.Sc., has served in the India Meteorological Department at Poona and Calcutta. He is the author of several original papers on geophysics and meteorology.

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A. C. CROMBIE, B.Sc., Ph.D., was appointed a lecturer in the Department of History and Philosophy of Science at University College, London, in 1946.

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C. D. DARLINGTON, D.Sc., F.R.S., was appointed Director of the John Innes Horticultural Institution in 1939. He is well known for his books, especially his *Evolution of Genetic Systems*, which established a new point of view in biology.

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W. P. K. FINDLAY, D.Sc., A.R.C.S., D.I.C., A.I.C.T.A., was appointed mycologist at the Forest Products Research Laboratory in 1927, and has since made an extensive study of wood-rotting fungi, with particular reference to their physiology.

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MICHELE GIUA has taught various branches of chemistry at Sassari University and in the Turin Academy of Artillery and Engineering. He has investigated molecular compounds, unsaturated compounds, and substitutions in the benzene nucleus, and is the author of many works on chemistry, notably the *Dizionario di Chimica Generale e Industriale*.

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W. GREY WALTER, M.A., Sc.D., was appointed Director of the Physiological Department of the Burden Neurological Institute, Bristol, in 1939. He is the co-founder and co-editor of the international journal *Electroencephalography and Clinical Neurophysiology*.

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D. R. HARTREE, M.A., Ph.D., F.R.S., M.I.E.E., was appointed to the John Plummer Chair of Mathematical Physics at Cambridge in 1945. In 1934 he was responsible for the installation at Manchester of the first differential analyser to be built outside the United States.

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SIR CYRIL HINSHELWOOD, M.A., D.Sc., Hon. D.Sc. (Dublin and London), F.R.S., is Dr Lee's Professor of Chemistry in the University of Oxford and a Fellow of Exeter College. He was President of The Chemical Society, 1946–8, and is well known as the author of several books on chemical kinetics.

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H. S. HOLDEN, D.Sc., F.R.S.E., F.L.S., has been Director of the Metropolitan Forensic Science Laboratory, London, since 1946.

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SIR MAURICE HOLMES, G.B.E., K.C.B., retired from the Board of Education in 1945, and has since undertaken various important administrative tasks on behalf of the Government.

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S. G. HOOKER, O.B.E., B.Sc., D.Phil., F.R.Ae.S., joined the staff of the Engine Division of the Bristol Aeroplane Company Limited in January 1949. He was formerly chief engineer of the Rolls-Royce works at Barnoldswick.

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J. W. HUNKIN, O.B.E., M.C., M.A., D.D., has been Bishop of Truro since 1935.

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G. O. JONES, M.A., B.Sc., Ph.D., has been a member of Professor Simon's Low Temperature Department of the Clarendon Laboratory, Oxford, since 1946. He has recently been appointed Reader in Experimental Physics in the University of London, at Queen Mary College.

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J. C. KENDREW, M.A., is a part-time member of the staff of the Medical Research Council and a Fellow of Peterhouse, Cambridge. He is particularly interested in the X-ray analysis of crystalline proteins.

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GEOFFREY LAPAGE, M.A., M.D., took up his present work on parasitology at Cambridge in 1930. He was formerly head of the Department of Zoology at University College, Exeter.

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L. C. LUCKWILL, B.Sc., Ph.D., has been a research pomologist at the Bristol University Agricultural and Horticultural Station, Long Ashton, Somerset, since 1945. He has made a special study of plant hormones in relation to horticulture.

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R. W. MARSH, M.A., has been mycologist at the Bristol University Agricultural and Horticultural Research Station at Long Ashton since 1926. His research has been chiefly on fungus diseases of fruit and their control.

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A. R. MCINTYRE, B.Sc., Ph.D., M.D., joined the University of Nebraska Medical College in 1932, and was appointed to the Chair of Physiology and Pharmacology in Omaha. With his collaborators he has investigated the effects of drugs and metallic ions on nerve and muscle mechanisms.

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A. R. MILLER, M.A., Ph.D., is particularly interested in statistical mechanics and thermodynamics. At the end of the last war he returned to Cambridge as an Imperial Chemical Industries Fellow, and is now working at the Royal Society Mond Laboratories on the problems of low-temperature physics.

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R. W. MONCRIEFF, B.Sc., F.R.I.C., F.T.I., has carried out research on cellulose esters, linear polymers, and the influence of molecular orientation on the physical properties of fibres. He is keenly interested in the relationship between chemical constitution and odour.

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H. MUNRO FOX, M.A., F.R.S., is Professor of Zoology at Bedford College in the University of London; he previously held the Chair of Zoology in Birmingham. His main research interest is in the physiology of aquatic invertebrates.

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L. N. OWEN, Ph.D., F.R.I.C., is Lecturer in Organic Chemistry at Imperial College, London. He is assistant editor of *Thorpe's Dictionary of Applied Chemistry*.

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W. H. PEARSALL, D.Sc., F.R.S., has been Quain Professor of Botany at University College, London, since 1945; previously he was Professor of Botany at Sheffield University.

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HANS PETTERSSON has carried out research in physics and oceanography, and was leader of the Swedish circumnavigating Deep Sea Expedition in the *Albatross*, 1947–8. Since 1930 he has been Professor of Oceanography at Göteborg. He has been Director of the Oceanographic Institute there since its foundation in 1930.

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F. A. PHILBRICK, M.A., is a teacher of English at St. Paul's School, Concord, N.H. Before taking up residence in the United States in 1939, and subsequently becoming an American citizen, he taught chemistry and physics at Clifton College and Rugby School.

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MICHAEL POLANYI, M.D., Ph.D., M.Sc., D.Sc., F.R.S., resigned the Chair of Physical Chemistry at Manchester University in 1948 to take up an appointment as Professor of Social Studies in the same university. Among his publications are many papers on the problems of science and society.

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F. E. SIMON, C.B.E., M.A., Ph.D., F.R.S., is Professor of Thermodynamics in the University of Oxford and in charge of the new Low Temperature Department of the Clarendon Laboratory. During the war he was a member of the technical committee in charge of the British atomic energy project, and was particularly concerned with experiments on isotope separation.

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SIR JOHN SIMONSEN, D.Sc., F.R.S., was appointed to the Chair of Chemistry, University College of North Wales, Bangor, in 1930, and held this post until 1943, when he was appointed Director of Research to the Colonial Products Research Council.

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J. YULE BOGUE, Ph.D., M.R.C.V.S., took up an appointment as physiologist with Imperial Chemical Industries in 1943. Previously he had been Lecturer in Physiology at the Royal Veterinary College, London, subsequently succeeding to the Chair of Physiology there.

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values of the specific heat which differ from those obtained by rapid cooling, and there are many other manifestations of the non-equilibrium, some of which we shall discuss later. We may mention, for instance, Tammann's [14] observation that by super-cooling liquids under different pressures and then releasing the pressure one can arrive at glasses with considerably different volumes.

We have in actual fact a practically infinite number of possible (but not equilibrium) states of the system; the glass has always a tendency to move from the configuration 'frozen-in' at a temperature in the transformation region, and corresponding to equilibrium at that temperature, towards that which would correspond to equilibrium at the temperature of its surroundings. Of course, the times required become longer and longer the further we go below the transformation region. This is quite different from the behaviour of a super-cooled, and therefore unstable, pure crystalline phase such as white tin, or diamond. We know that although diamond is unstable under ordinary conditions, with respect to graphite, it nevertheless remains in the diamond configuration for practically infinite times, and such persistence of a metastable crystalline state is quite common. Such phases, however, are in *internal* equilibrium; each slight deviation due to fluctuations only leads back to the original state, since all neighbouring states are less stable. Each atom lies in a potential-energy trough, and the whole structure lies in a free-energy trough—though not the lowest possible. In the glass, however, fluctuations tend on the average to lead it away from the chance frozen-in configuration towards that corresponding to equilibrium. The atoms lie in potential-energy troughs of differing depths; there are always some in troughs of practically zero depth, or at least in troughs bounded on one side by walls of practically zero height. The structure as a whole thus no longer lies at the bottom of a free-energy trough, but is, so to speak, stuck at some point on one of its sides.

It is thus not the fact that a glass is a super-cooled, unstable phase which is characteristic for it, but the fact that it is a super-cooled *disordered* phase. It has a chance configuration, frozen-in at some temperature in the transformation range, and only one of a continuous series of possible disordered configurations.

We can of course apply thermodynamic considerations to, say, the transition from graphite to diamond, because we can think of a mechanism by which we could establish an equilibrium between the two states at any temperature—for instance by increasing the pressure [15]. The glass, however, is not in internal equilibrium; we cannot even think of any method by which we could establish an equilibrium between the glass and crystal and therefore be enabled to transform the crystal reversibly into the glass, at temperatures below the freezing-in region. In other words, we cannot, starting from a given temperature, aim at and reach the configuration corresponding to a higher temperature. Glass is not a 'thermodynamic' phase, and we cannot, for instance, apply the Second Law of Thermodynamics to calculate the vapour pressure, since a vapour pressure cannot build up unless configurational adjustments are possible, at least on the surface. Such a phase has no definite vapour pressure—it changes with time.

We can still speak of the entropy of a glass at the absolute zero from the statistical point of view,[1] just as we can speak of the entropy of a gas disturbed by ventilators. We cannot, however, expect to apply thermodynamic laws to these states, or at least to any properties connected with changes in the internal positions or orientations of the molecules. However, if we consider properties which are affected only by the vibrations of the molecules, such as the volume as measured over short periods, we can naturally expect to apply the laws of thermodynamics. Indeed, we find experimentally that one requirement of the Third Law, namely, the disappearance of the thermal expansion with the approach to absolute zero, is fulfilled.

Thus considerations arising from the Third Law of Thermodynamics have led us to a clarification of the nature of glasses. But the results of these investigations have also led to a clarification of the meaning of the Third Law. Previously, many scientists denied the Third Law any general validity [16]. We know now that entropy differences do not necessarily disappear between all conceivable states at the absolute zero, but only between those which are in internal equilibrium. Thus the Third Law is a general law of *thermodynamics*. One of us [17] has therefore given it the following formulation[2]: 'the contribution to the

[1] An estimate of this quantity was actually made by Pauling and Tolman [21] many years ago by the methods of statistical mechanics, that is, by considering the number of possible atomic configurations in the glass.

[2] This formulation has been accepted without essential change in the latest edition of *Statistical Thermodynamics* by Fowler and Guggenheim.

entropy due to each factor within the system which is in internal equilibrium becomes zero at absolute zero.'

Many similar systems are now known in which an arrangement stable at higher temperatures can be super-cooled, such as solid solutions, in which either an excess concentration of one component, or an abnormally high degree of disorder, can be retained by super-cooling. The ortho-para hydrogen system is another example. In modern low-temperature research we often come across systems in which some part, such as for instance the electron spin system, or the nuclear spin system, can correspond to a temperature quite different from that of the lattice vibrations. We can easily imagine systems, say a glass containing rare earth ions, and of course nuclear momenta, which could only be characterized by four different 'temperatures,' and a corresponding number of relaxation times.

The freezing-in phenomenon has more obvious and direct bearing in our case, however, than in most of the above examples. First, the quantities of entropy involved are larger. In the order-disorder transition in binary alloys, for instance, the total change in the configurational entropy is of the order of R (the gas constant) multiplied by the logarithm of a small number, while the changes with which we are concerned can go up to, say, $R \log_e 1{,}000$. Secondly, the change in order in liquids affects the whole structure of the material, whereas in the other order-disorder transitions mentioned the lattice points are more or less fixed, and it is only in the orientation or some other characteristic of the occupying atoms or molecules that there is disorder.

PROPERTIES OF GLASSES

We must now consider some of the practical implications of the unique position of glasses in the thermodynamic hierarchy. The most obvious and important fact is that an ideal glass is homogeneous to a much finer order of size than, for example, a polycrystalline metal. Formed by continuous cooling from the liquid, it possesses no 'structure' of greater size than that of the short-range order characteristic of a liquid. In other words, we can think of a lump of glass, just as of a single crystal, as being one large molecule. To this we can attribute one of the most important properties of glass—its transparency—since this must mean that there are no crystalline inclusions, and also no internal surfaces or holes, of size approaching the wavelength of light. It also affects the thermal conductivity profoundly, especially at very low temperatures, as Berman [18] has recently shown in the Clarendon Laboratory at Oxford.

We can also attribute the various mechanical qualities of glass—good or bad—to these facts, coupled with the fact that glass in ordinary use is not in internal equilibrium. The behaviour of glass under stress is analogous to that of an ordinary liquid, its yield point being at zero stress. Fortunately, however, the viscosity may be very high. There can be no slip along glide planes as in a single crystal, or as to a reduced extent in a polycrystalline material, but on the other hand these materials have the advantage of a very low rate of deformation, if any at all, below a critical value of the stress. It is therefore an interesting fact that glass is actually used as a *structural* material for applications which call for the greatest degree of rigidity, e.g. in the manufacture of the largest astronomical mirrors. Also, it is a matter of common experience that ordinary glasses are among the hardest of materials.

Naturally great care has to be taken to reduce to the minimum those effects which are due to the internal non-equilibrium. The configurational contribution to the value of any property usually has the same sign as the vibrational contribution, since an increase in either temperature means greater disorder.[1] A rapidly cooled piece of glass, which has a high configurational temperature, therefore has an abnormally large volume. It also has a relatively low value of viscosity, and a consequently more rapid rate of approach to equilibrium than slowly cooled glass. To achieve the greatest stability, therefore, the configurational temperature must be lowered as far as possible. Glass apparatus required for precision measurements, such as mercury-in-glass thermometers, is heated for a long time at a temperature somewhat below the normal transformation region for this purpose. In the case of optical glass it is particularly important that the configurational temperature should be the same everywhere, so it is essential that temperature gradients should be reduced to a minimum during cooling through the critical freezing-in region.

The very erratic behaviour of glass in respect of mechanical strength can again be attributed to the fact that it is, so to speak, one large molecule. It is generally believed that the breaking process

[1] This is not always true, and there are important exceptions to the rule in the behaviour of silica, water (which has recently been shown by the authors [22] to form a true glass), and certain alloys.

in all solids is precipitated by stress-concentration at flaws or weak places. In glass, a single crack starting at the worst flaw is able to spread right across the specimen. In a polycrystalline material, however, as pointed out by Orowan [19], it is difficult for a crack to break through from one crystal into a neighbour with different orientation, and the strength as determined is more of an indication of the average strength of all the crystals. There are also several possible mechanisms by which flaws can arise in glass which cannot play a part in crystalline material (though in this respect it has one advantage over the crystal, in which solidification occurs discontinuously). For instance, the fact that at room temperature ordinary glass has a configuration frozen-in from a much higher temperature (nearly three times as high a temperature on the absolute scale) means that the structure will contain 'holes' formed by thermal fluctuations which will correspond to the higher temperature [20].

The looseness of the essentially liquid-like structure of glass is also reflected in its porosity to sufficiently small particles, such as the smaller gaseous atoms and molecules, and sodium and other ions. This is made use of in the well-known glass electrode and in the manufacture of 'Vycor' glass. It is worth noting that the porosity below the transformation region can be reduced by prolonged heating, which tends to close up the structure.

CONCLUSION

Finally, a few remarks about states of aggregation. The glassy state has sometimes been called a 'fourth state of aggregation.' If we agreed to this definition we should have to class as additional states of aggregation many of the other examples mentioned above of frozen-in disordered systems. The lack of precision in this field is only due to the looseness of the definition of a state of aggregation. If we want a practical or popular definition, we can say that a solid body is something with which one can smash someone's head, a liquid is something in which one can drown, and a gas something which has to be kept in a completely closed container. From this point of view glass must certainly be classed as a very useful solid. Then there are definitions of the states of aggregation from purely molecular standpoints. The limits covered by the two kinds of definition certainly do not coincide. There are very soft crystal lattices and there are very hard disordered phases, such as the glasses. It is well known that a continuous change is possible between the liquid and gaseous states, and obviously in this case there can be no point in trying to define the exact limit of either 'state of aggregation'; this also invalidates any definition of the term 'state of aggregation' based on the existence of discontinuities between phases. Indeed, the possibility of a continuous series of states between solid and liquid cannot be excluded, although it has not yet been realized in practice. As usual, it is futile to try to force the whole variety of nature into the Procrustean beds of a few names. If we find difficulty in allocating glass to one of the conventionally defined states of aggregation it is not because we know too little about it, but because we know too much.

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The floor of the ocean

HANS PETTERSSON

Fuller knowledge about the floor of the ocean would enable geologists to explain much of the Earth's earlier history. Within recent years greatly improved methods of oceanography have been developed, and with the new core-samplers, hydrophones, and ultrasonic depth-recorders it has been possible to amass a wealth of valuable data. Professor Pettersson here describes some of the results obtained, especially those from the very successful *Albatross* expedition.

Some geologists have asserted—though with undue pessimism—that in a few thousand million years the Earth will have become completely desiccated and as bare of oceans as Mars is at present. Let us assume that this tragic fate—which will deprive our planet not only of its oceans but also of its oceanographers—has been half realized, so that the ocean surface has become sunk by about 2,000 fathoms. The Atlantic Ocean will then appear divided in two, the halves being separated by a mid-Atlantic continent, with a crest constituted by land which is now some 1,500 fathoms below the surface and is called by oceanographers the Central Atlantic Ridge. This is illustrated in figure 1, where the present continental surface is grey, the ocean remaining after the world-wide sinking of the sea surface is black, and the border-land left dry by the retreating water is white.

The deep ocean floor, which would still be under water after such a partial desiccation, is the least-known part of the Earth's crust, the *terra incognita* of oceanography, geology, and biology. Until 75 years ago it was practically unexplored; then the famous cruise of the *Challenger* (1872–6) threw it open to science. Owing to great technical difficulties, the progress made since then in studies of the ocean depths has been slow and laborious. As an example, we may take the sampling of deep-sea deposits by means of sounding tubes. During the half-century which passed between the expeditions of the *Challenger* and of the *Meteor* (the German Atlantic expedition of 1924–5), the maximum length of core raised from great depths increased only from two feet to three. This slow advance is the more to be regretted since the deep-sea sediments, accumulated through hundreds of millions of years, harbour a unique record of the main catastrophes—climatic, tectonic, and volcanic—which have passed over the Earth (figure 5). A close study of these archives of the deep would spread light over many obscure chapters in the Earth's past history.

During the second world war, Swedish oceanographers, cut off from active work at sea, devoted their efforts to improving the tools and methods used in studying the floor of the ocean. A first result was the vacuum core-sampler, which in 1942 gave us a core of 45 feet from the depth of the Gullmar Fjord. Three years later, Dr B. Kullenberg, by means of his piston core-sampler, succeeded in obtaining a core 65 feet long (figures 7 and 10). This was a sevenfold increase over the record length

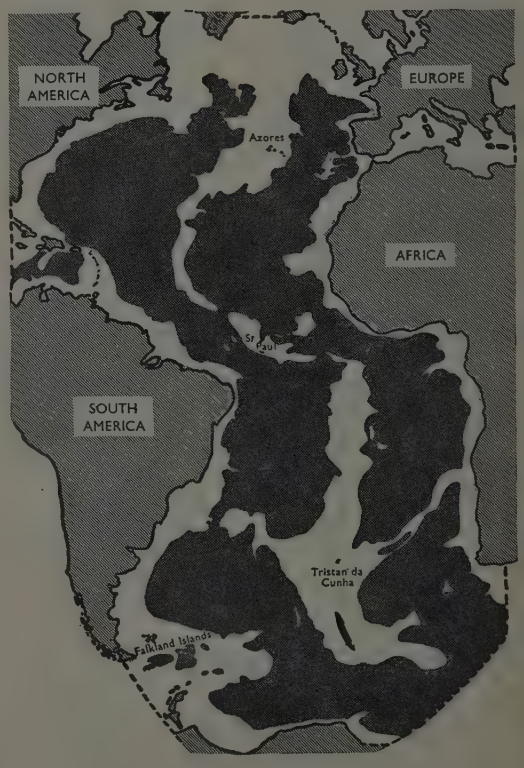


FIGURE 1 – *The Atlantic Ocean on a half-desiccated earth.*

FIGURE 2 – *The* Albatross *under sail.*

FIGURE 3 – *Large waterbottle for sampling sea-water for radium and uranium.*

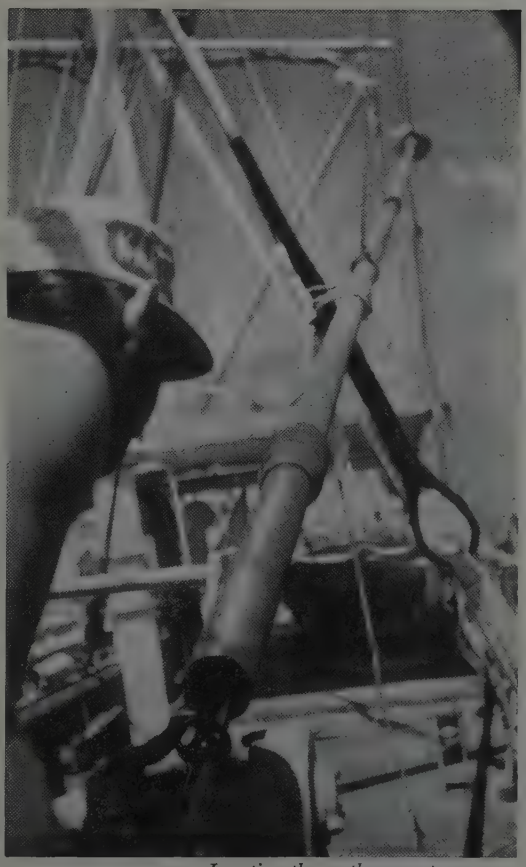


FIGURE 4 – *Inserting the geothermometer.*

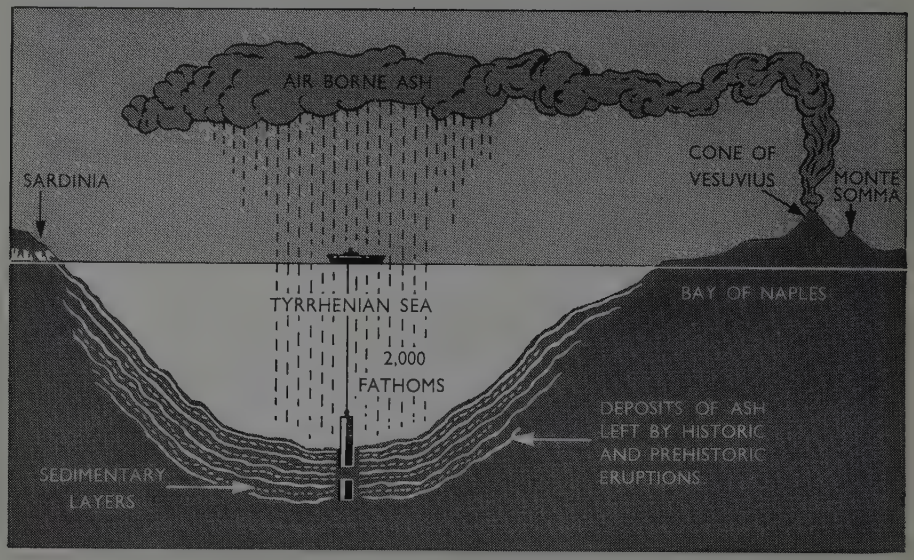
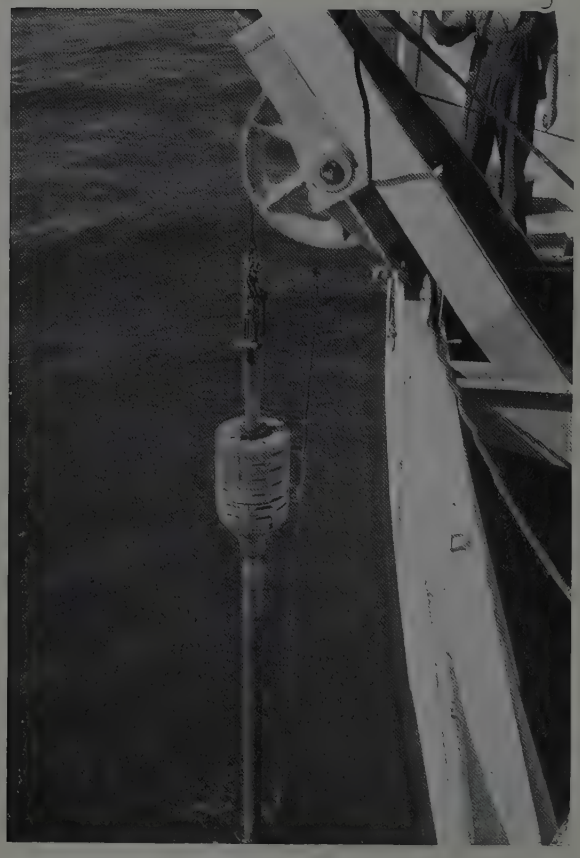


FIGURE 5 – *Ash-rain from Mount Vesuvius settling to the bottom of the Tyrrhenian Sea.*



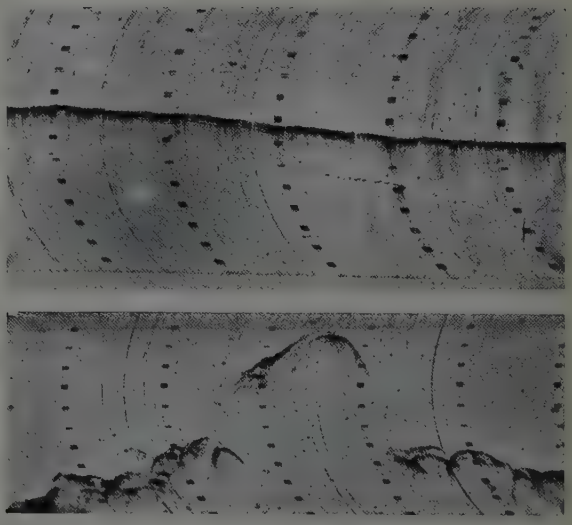


FIGURE 6 – *Echogram of (above) smooth and (below) rough bottom of the ocean in 2,500 fathoms. Depth scale 10 fathoms per scale unit; distance between two arcs about 1 nautical mile.*

FIGURE 7 (*left*) – *The piston core-sampler ready to descend to 4,000 fathoms.*

of core obtained ten years earlier by means of an explosive core-sampler, invented by Dr C. S. Piggot. Figure 8 shows the development in core-length during 75 years. The span of time represented by the individual cores given in the figure is based on an assumed rate of accumulation of the Atlantic red clay of 7 millimetres in 1,000 years.

Another notable achievement was that of Professor W. Weibull, of the Bofors Armament Works, who succeeded in developing a method for measuring the thickness of the sediment carpet over the ocean floor. By recording, with hydrophones of special construction, the echoes from exploding depth-charges, reflected against the bottom and against the 'bottom below the bottom,' Weibull could locate reflecting surfaces covered by many thousands of feet of sediment. Other improvements were made in determinations of the radioactive elements uranium and ionium, present in seawater and in the sediments; in studies of the strength and the spectral composition of submarine daylight; and in investigating the occurrence of rare particles suspended in the water from great depths. A novel type of ultra-sonic depth-recorder was constructed for the Swedish expedition by a British firm (the Marine Instruments Company of London). Under favourable conditions of wind and ocean-swell, it drew a detailed curve showing the bottom profile down to depths exceeding 4,000 fathoms.

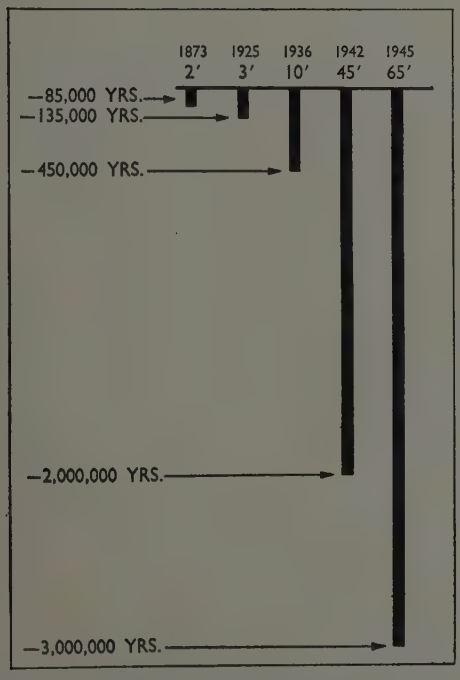


FIGURE 8 – *Diagram showing improvements in coring technique during the past seventy-five years.*

After a test cruise to the western Mediterranean in the spring of 1946, when the new technique was thoroughly tried out, the Swedish deep-sea expedition started on its circumnavigating cruise from Göteborg on 4th July, 1947. It was entirely financed by private donations given to the Royal Society of Göteborg, which sponsored the cruise. The splendid new training ship *Albatross*, a motor-schooner of 1,450 tons dead weight (figure 2) was lent to us, for our expedition of 15 months, by the Broström shipping combine of Göteborg on very generous terms. For the purpose of the cruise it had been converted into a floating laboratory at the Lindholmen shipyard (figure 11). A unique, very powerful, electrically driven deep-sea winch, with 4,400 fathoms of tapered steel wire rope made it possible to carry out coring and deep-sea trawlings with a minimum loss of time. Both operations were admirably conducted by Dr Kullenberg. The ship was under the command of Captain Nils Krafft. The scientific staff consisted of 10 to 12; they were specialists in the work to be performed.

Owing to the need for fair weather for working our very heavy gear the course to be followed was laid within or near the doldrums, where, fortunately, both the water layers and the sediments underneath present problems of very great interest; see the map in figure 9. In order to maintain a high standard of efficiency on the part of the staff in a tropical climate, the laboratories as well as the cabins and mess rooms were air-conditioned.

Following an almost straight course from Madeira to Martinique, raising long cores and sounding the sediment layers under way, the *Albatross* reached the Panama Canal six weeks after the start. On 27th August, 1947, the expedition set out from Balboa on its Pacific cruise, which lasted for nearly five months. A zigzag course was followed, which afforded opportunities of taking four complete oceanographic sections across the equatorial current system (figure 9). On the way, the Galapagos Islands, Nukuhiva (in the Marquesas), Tahiti with Papeete, and Oahu with Honolulu were visited, as well as the interesting atoll of Kapingamarangi. Attempts, partly successful, at measuring the unknown geothermal gradient in the deep-sea deposits were also made by means of a new geothermometer designed for great depths (figure 4).

FIGURE 9 – *The course of the* Albatross *through three oceans.*

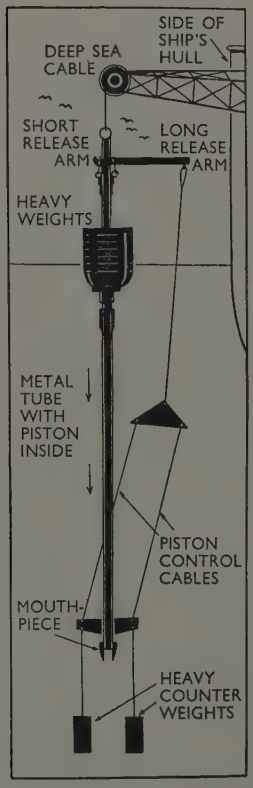


FIGURE 10 – *Schematic drawing of the piston core-sampler.*

After working in the great deep S.E. of Mindanao the *Albatross* passed through the Sunda Sea, with its many active volcanoes, to Surabaya, Java, and thence, via Bali, into the Indian Ocean. Here the only gale of the cruise occurred; it gave us four rather uncomfortable days. In the Indian Ocean the route lay mainly south of the equator, with a northerly detour to Colombo, and from there to the Seychelles, the 'Paradise of the Indian Ocean.' Proceeding through the Red Sea and the Mediterranean the expedition near the end of May 1948 reached Monaco, the Mecca of deep-sea research since the days of its great oceanographer Prince Albert I.

The last four months of the cruise were spent in the Atlantic Ocean, down to the Romanche Deep and St Paul's Rocks on the equator, and over to the Virgin Islands in the West Indies. During this part of the cruise Dr O. Nybelin, of the Natural History Museum of Göteborg, made rich catches of fishes and invertebrate bottom animals by means of the deep-sea trawl. His deepest haul, made to the N.E. of the Virgin Islands, proved that even at a depth of more than 4,200 fathoms organic life exists.

On our way back to Göteborg, where the *Albatross* arrived on 3rd October, 1948, we had the pleasure of visiting the Port of London and showing our equipment and our results to our British colleagues who visited the *Albatross*.

The main results gained during the expedition may be summarized as follows:

1. Down to the greatest depths encountered, the bottom profile is much more rugged than has generally been assumed, a large stretch of flat bottom being rather the exception than the rule. This fact is a serious complication, both in coring operations and in deep-sea trawling (figure 6).
2. Hard bottom is not of infrequent occurrence, especially in the Pacific and the Indian Oceans. It is largely an effect of submarine volcanism having spread extensive lava beds over the ocean floor, some of them covered by only a thin veneer of sediment.
3. Depth-charge soundings of the thickness of the sediment carpet gave much higher values in

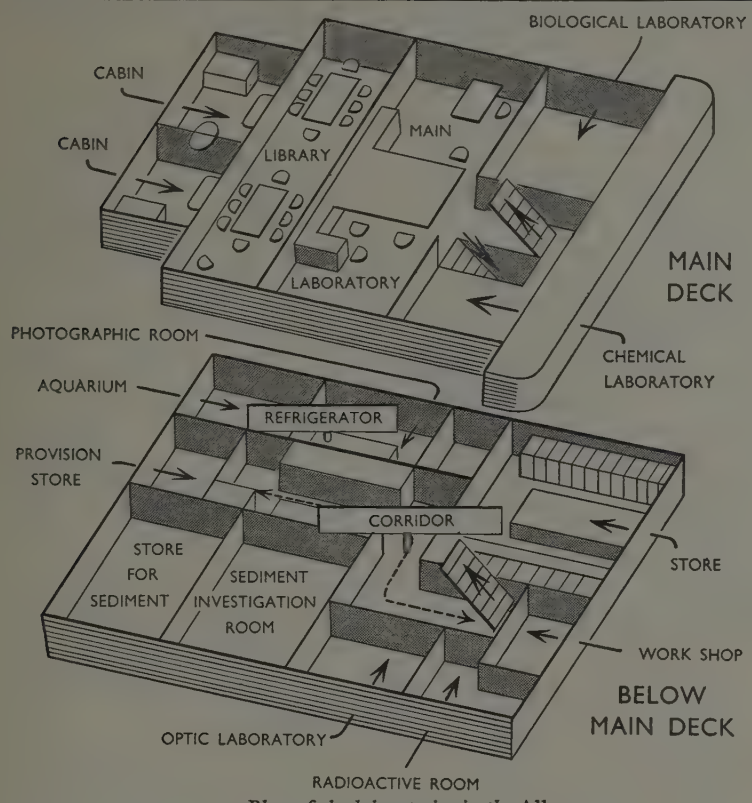


FIGURE 11 – *Plan of the laboratories in the* Albatross.

the Atlantic Ocean,[1] the Caribbean, and the Mediterranean, than in the Pacific and the Indian Oceans. It is possible that these latter results may be due to intermediate lava beds reflecting the explosion waves.

4. The sediment cores raised from great depths in all three oceans, ranging in length up to a maximum of 60 feet, made up a total length exceeding an English mile. They are now being subjected to various analyses in the laboratory. Even the cursory examination made on board showed many of them to be stratified, which indicates that variations in the conditions of sedimentation have taken place during the span of time represented by the length of the cores, from some ten thousand up to a few million years. Some of the changes are due to climatic variations, i.e. to the alternation of glacial and interglacial periods characteristic of the Quaternary Age. These variations can be followed by means of a biological analysis of the calcareous foraminifera shells found in different levels of the core. A surprising find was that of sand, or coarse mineral fragments, which blocked the core samplers both in the Mindanao and in the Romanche Deeps. Indications of 'slumping,' i.e. submarine land-sliding, were also encountered.
5. In the Mediterranean, the Caribbean, and the Sunda Seas numerous ash-layers from volcanic eruptions were found intercalated in the ordinary sediment. Organic remains and humus were of fairly frequent occurrence, especially in the seas just mentioned and in the open Atlantic west of St Paul's Rocks, as well as in the Bight of Panama.
6. A good catch of manganese concretions, some of them of considerable size, was made at a depth of about 3,000 fathoms S.E. of the Bermudas. Determinations of their rate of growth by measurements of the radium content are in progress.

When these abstracts from the records of the deep have been thoroughly studied by physical, chemical, radioactive, mineralogical, and biological analyses, much more will be known about the ocean floor and its deposits. It is to be hoped that this cruise, in which novel methods of deep-sea research have been successfully used, may be followed by others, and that the future study of the floor of the ocean will be based on international co-operation.

[1] The maximum value of nearly 10,000 feet would, with the assumed rate of the accumulation of red clay, represent a time-span of 300 to 400 million years, possibly still more.

Fruit development in relation to plant hormones

L. C. LUCKWILL

Recent work on plant hormones has shown that these substances play a vital role in the initiation and control of fruit growth. They are also known to control the process of abscission, which is the immediate cause of fruit-drop in apples and similar fruits. In this article, the importance of developing seeds as centres of hormone production in the fruit is stressed, and it is shown that appreciation of this importance has led to a fuller understanding of many previously known facts relating to fruit-development and fruit-drop.

The initiation of fruit-development in plants follows closely upon the act of fertilization. It manifests itself by a sudden renewal of growth in the ovary and adjacent tissues, the particular tissues involved varying widely in different plants. Thus, in the plum and tomato the fruit is developed entirely from the ovary itself; in the strawberry the edible portion of the fruit is derived from the floral axis, and the same is true for the apple and other pome fruits; in the mulberry and the pineapple it is the perianth segments of the flowers which enlarge and become fleshy; and in the pomegranate it is the outer coats of the seeds themselves.

The stimulus to growth which follows fertilization is now generally recognized to be hormonal in nature, though there is still some doubt as to the exact source of the hormone concerned. The pollen of many plants is known to contain growth hormones, and, as long ago as 1909, Fitting [1] at Buitenzorg demonstrated that aqueous extracts of the pollen of certain orchids were capable of inducing a limited amount of growth when applied to orchid ovaries. More recently, other workers have succeeded in stimulating fruit-development by injecting ovaries with extracts prepared from the pollen of quite unrelated species [2]. In nature, although a slight swelling of the ovary is sometimes observed after pollination, it is only in exceptional cases that pollination alone, unaccompanied by subsequent fusion of the male and female gametes, results in full development of the fruit.

On the other hand, young seeds, shortly after fertilization, have been shown to be exceptionally active centres of growth-hormone production, and it seems very likely that it is this hormone which is responsible for the initiation of fruit-growth in many plants. In maize, no detectable hormone is present in the developing ovule, but immediately following gametic union hormonal activity appears, reaching its peak within ten to fifteen days and thereafter decreasing again [3]. A similar cycle of accumulation and disappearance of growth-hormone following fertilization has been observed in other cereals. In rye [4] and in apple [5] it has been demonstrated that the seed hormone originates, not in the growing embryo, but in the nutritive endosperm tissue of the seed. This tissue, like the embryo itself, is formed as a result of a sexual fusion in the embryo sac. In other plants, such as the tomato, the concentration of hormone in the young seeds is found to be very much higher than in other parts of the fruit [6], which again suggests that the seeds are the chief centres of hormone production.

That growth-hormones are in fact necessary for, and capable of stimulating, fruit-development has found confirmation in recent work on synthetic growth-substances. These substances are known to produce effects on growth similar to those produced by natural plant hormones, and, in certain plants, they are active in stimulating fruit-development in the absence of fertilization. A dose of only 0·001 μg of 2 : 4-dichlorophenoxyacetic acid applied to an unpollinated tomato ovary is sufficient to initiate fruit-development, and a dose of 0·1 μg results in the development of normal sized fruits. Other phenoxy- and naphthoxy-acids show similar fructigenic activity — though at somewhat higher dosage levels — and certain of these compounds are now widely used in commercial tomato-growing for setting the fruit under conditions when natural pollination is deficient. In the tomato, the fruits induced by growth-substances are seedless (figure 1), but in melons, *Datura*, *Melandrium*, *Oenothera*, and other plants, treatment with growth-substances stimulates not only the

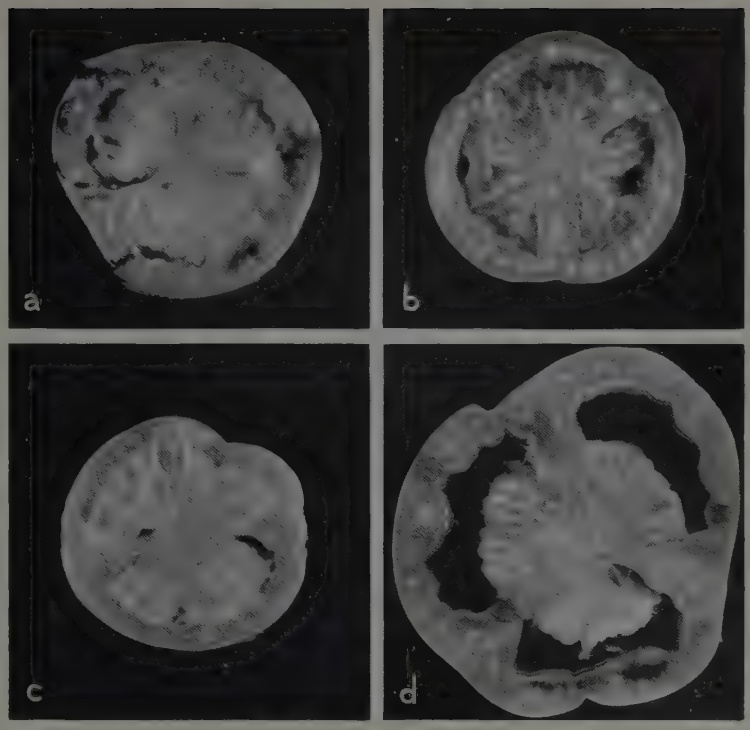


FIGURE 1 – *Transverse sections of tomatoes:* (a) *normal seeded fruit produced by natural pollination;* (b)–(d) *parthenocarpic fruits produced by treating unpollinated ovaries with* (b) *2-naphthoxyacetic acid,* (c) *an extract of apple seeds and* (d) *2:4-dichlorophenoxyacetic acid.*

FIGURE 2 (*below*) – *The effect of seeds on fruit development is well illustrated by the strawberry. Where pollination is inadequate few fertile seeds are formed and irregular misshapen fruits like those on the left result. Notice that the fruit flesh has swelled only in the areas adjacent to the fertile seeds. The fruit on the right has been sprayed with a synthetic growth substance* (α-*naphthalene acetic acid*) *which ensures full development of the flesh, even though few seeds are present.*

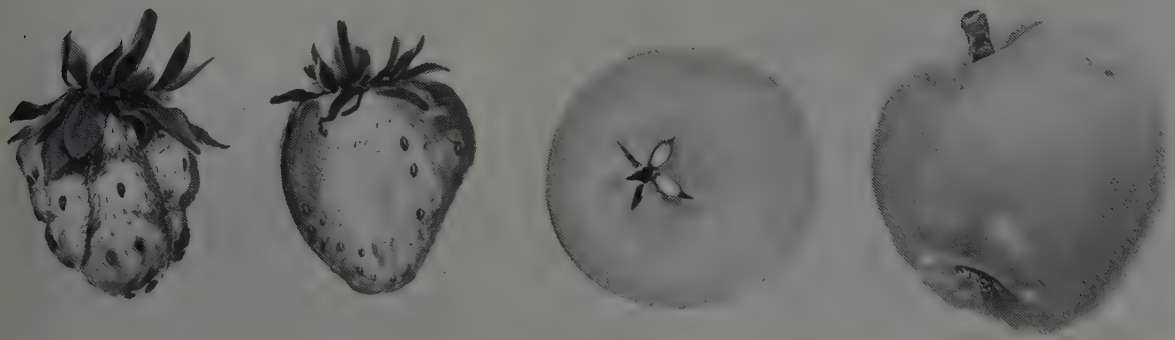


FIGURE 3 (*above*) – *Asymmetric development of apples and other pome fruits results from failure of seed development in one or more of the five carpels.*

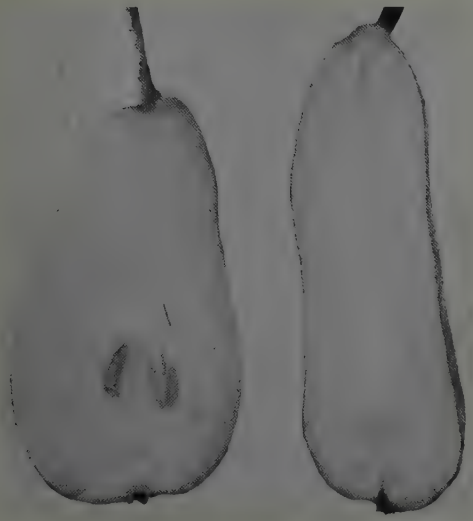


FIGURE 4 (*extreme left*) – *Parthenocarpic* (*seedless*) *pears are of fairly frequent occurrence in many varieties, and differ markedly in shape from seeded fruits of the same variety.*

FIGURE 5 – *An early stage in the abscission of a plum fruitlet. A plate of meristematic cells* (*A–A'*) *has formed across the cortex and is spreading across the vascular strands.* (*Magnification* × 57.)

FIGURE 6 – *A later stage in the abscission of a plum fruitlet. The abscission layer (A–A′) extends across the base of the fruit stalk, and separation of the stalk from the parent spur has already started at A′.* (*Magnification* × 37.)

FIGURE 7 – *Longitudinal section of apple ovule at the time of fertilization. In the centre of the embryo-sac the fusion of one of the male gametes with the embryo-sac nucleus can be seen. It is from this fusion nucleus that the hormone-producing endosperm tissue is developed.* (*Magnification* × 167.)

FIGURE 8 – *Growth of apple seed showing successive stages in endosperm development.* (a) 24 *days after fertilization. The endosperm is still in the free-nuclear condition.* (b) *At* 31 *days the endosperm has become cellular and completely fills the elongated embryo-sac.* (c) 45 *days. The endosperm is enlarging rapidly by meristematic activity at the periphery, but at the same time is being digested by the rapidly growing embryo.* (d) 76 *days. The embryo is now fully grown and is enclosed in a sheath of active endosperm cells. E = endosperm.* (*Magnification* × 12.)

Figures 1 to 8 are reproduced by courtesy of the Director, Long Ashton Research Station.

fruit but the seed coats [7]. Such seeds, although normal in external appearance, never contain embryos and are thus incapable of germination.

Although it seems that the hormone stimulus responsible for the initiation of fruit-development in many plants originates in the fertilized ovules, there are certain facts which suggest that this may not always be so. In the apple, for instance, no hormone can be detected in the young seeds until three weeks after the fall of the petals, yet during this interval the fruits swell rapidly [5]. Moreover, it is well known that some plants are able to form their fruits even though fertilization and seed development do not occur. This phenomenon, which is known as parthenocarpy, is particularly common among cultivated fruit plants, many of which have been specially selected for their ability to produce such seedless fruits. The banana, the Washington navel orange, and the Corinth (currant) grapes are well known examples. Even in plants which normally produce seeded fruits, parthenocarpy may occur spasmodically and may often be induced by external agencies. Conference and Fertility pears will often set a good crop of seedless fruits after the flowers have been damaged by frost, or when they are grown in a very warm climate, and seedless gooseberries, grapes, and apples have been produced experimentally by cincturing. In such examples, it is clear that the hormone responsible for fruit-development cannot come from the developing seeds, but must originate in the maternal tissues of the ovary or adjacent vegetative organs. Seedless varieties of oranges and lemons have been shown by Gustafson [8] to have an exceptionally high concentration of growth hormone in their ovaries, as compared with related varieties the fruits of which will develop only when seeds are present. This high hormone content appears to be adequate to ensure the continued development of the ovary into a fruit, even in the absence of seeds. The occasional occurrence of parthenocarpy, therefore, in no way invalidates the general hypothesis that the young seeds are the active centres of production of the hormone responsible for the initiation and control of fruit-development.

This hormone concept of fruit-development has thrown new light on several other phenomena for which there had previously been no satisfactory explanation. The influence of the seeds on the size, shape, and chemical composition of fruits has long been known; it is especially marked in apples and pears and other few-seeded fruits. A particularly striking correlation between seed number and fruit size is quoted by Müller-Thurgau [9] for grapes (figure 9), and similar correlations are reported for a wide variety of other fruits.

The influence of seeds on the shape of fruit is particularly well shown by the pome fruits, where failure of seeds to develop in one or more carpels results in asymmetric growth of the flesh (figure 3). In strawberries too, where only a few fertile seeds are formed, the swelling of the fruit is irregular, and is confined to the areas immediately adjacent to the seeds (figure 2). As in apples, the influence of the seed here seems to be strictly local in its effects. Parthenocarpic fruits often differ from seeded fruits of the same variety, not only in size and shape but in chemical composition. In pears, they are characteristically sausage-shaped (figure 4); in the Japanese persimmon they are smaller, earlier in ripening, and with darker flesh than seeded fruits of the same variety [10].

The phenomenon of metaxenia, or the influence of pollen on the character and growth of fruit, has for long been a subject of controversy among pomologists. Formerly, indeed, it was difficult to visualize how such effects could occur, since the fruit itself, with the exception of the content of the seeds, is entirely a product of maternal tissue. Many so-called metaxenia effects have, in fact, been shown to be due to somatic mutation, but

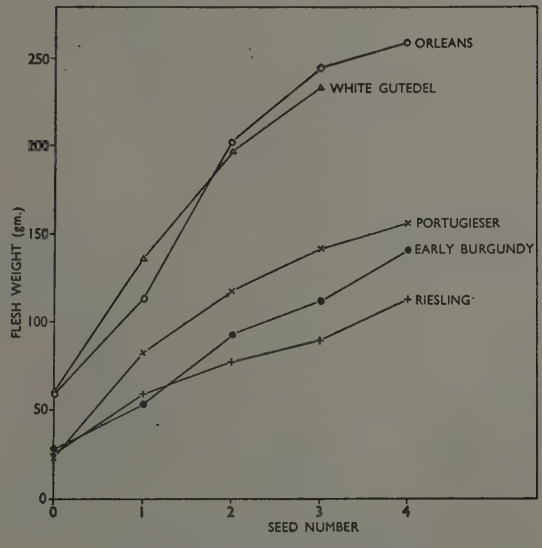


FIGURE 9 – *The correlation between seed number and fruit size is particularly well marked in few-seeded fruits, and is illustrated by the above graphs of flesh-weight in different varieties of grapes, based on the data of Müller-Thurgau.*

there nevertheless remain some well authenticated cases where the size, chemical composition, and keeping qualities of the fruit have been affected by the variety of pollen used. If it be accepted that fruit-development is under the control of a hormone produced in the seeds, the possible mechanism of such effects becomes apparent, since the quantity, and possibly the type, of hormone produced may well be influenced by the hereditary make-up of the endosperm, to which, of course, the male parent contributes.

In some plants it is usual for every flower which is effectively fertilized to develop into a mature fruit. This is so in the gooseberry, the cucumber, and the tomato. In species which flower abundantly and have large fruits, however, a large proportion of the fruitlets which start to develop are shed before they reach maturity. Such shedding is clearly necessary if impossible demands are not to be made on the nutritional and mechanical capabilities of the parent plant.

In the cultivated tree fruits the problem of fruit-drop is one of great economic importance, since the extent of the drop is often one of the chief factors determining the size of the harvested crop. In apples and plums, in which the phenomenon has been most intensively studied, the most characteristic feature of fruit-drop is its periodicity, successive waves of dropping being separated by 'sticking periods' during which few fruitlets fall. In the apple there are usually three waves of dropping: the first, or post-blossom, drop occurs soon after petal-fall, and is followed after an interval of one to three weeks by the so-called June drop; finally comes the pre-harvest drop, which, as its name suggests, is the fall of nearly ripe apples. In plums, too, three waves of fruitlet-drop can usually be recognized [11], though these are not necessarily homologous with those in the apple. From the economic standpoint, the early waves of fruitlet shedding are to a large extent desirable, as they represent a natural thinning-out of overcrowded fruit spurs, a process which otherwise would have to be carried out by hand. In some varieties, however, excessively heavy June drop frequently occurs, with consequent poor cropping, whereas in others the pre-harvest drop regularly accounts for large losses to the grower.

The separation of young fruitlets from the parent spur is an abscission process essentially similar to that responsible for the fall of leaves in the autumn. The first stage in the abscission of a fruit is the formation of a plate of meristematic cells across the base of the fruit stalk, or, occasionally in plums, at the base of the fruit itself (figure 5). This abscission layer, which may be anything from two to ten cells in thickness when fully formed, extends across all the living tissues of the stalk. The xylem elements which pass through the layer are stretched like a spring, and often break, leaving fragments within the zone (figure 6). The actual separation appears to be due primarily to chemical changes in the cell walls within the abscission zone, the cellulose of the wall changing into gelatinous pectic compounds. This results in a zone of weakness which is readily ruptured by the mere weight of the fruitlet. The scar left after the fruitlet has fallen is soon healed over with corky tissue developed from a secondary cambium below the original absciss layer [12].

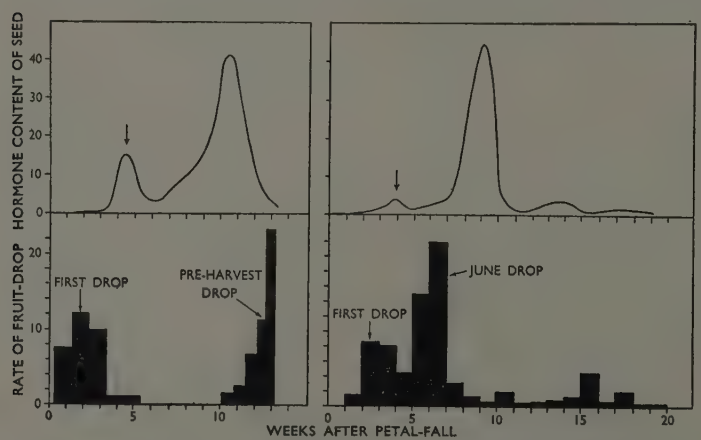


FIGURE 10 – *Relation between the hormone content of the seed and the rate of fruit-drop in the apple varieties Beauty of Bath (left) and Lane's Prince Albert (right). It will be noticed that the main peaks of hormone production are associated with periods of low fruit-drop. The arrows mark the stage at which the endosperm tissue becomes cellular.*

Although the anatomical features of the abscission process are fairly well understood, the physiology and the underlying causes of fruit-drop are still only imperfectly known. The earlier pomologists laid stress on the importance of water relations, and Langley [13] in 1729 wrote:

> 'Tis very easy to conceive, that if very dry Weather exhales away that Moisture which is necessary for those Formations (the internal parts of the fruit, such as the kernels, stones, and

the like) the Work will be imperfect, and consequently the Fruits must perish.'

Since the days of Langley, much evidence has accumulated to show that restricted water supply during the early stages of fruit growth does, in fact, promote abscission; but, at the same time, it has become clear that water is only one of many factors which influence the drop. Nutritional factors also are of great importance, and a number of investigators have drawn attention to the close correlation which exists between the leaf area of the fruiting spur and the number of fruitlets which are retained. More recently, work on plant hormones has opened up a new aspect of the phenomenon of abscission, and seems to have brought the problem one stage nearer its final solution.

In many plants, it can readily be demonstrated that the formation of an abscission layer at the base of the leaf stalk is under the control of a hormone-like substance manufactured in the leaf blade. Removal of the leaf blade results in the rapid formation of the absciss layer at the base of the stalk, but, if the cut end of the stalk be supplied with growth substance, abscission is prevented. A similar experiment can be performed with apple fruits, suggesting that here, too, the abscission process is under hormone control. Growth substances applied in the form of sprays are also effective in delaying fruit-drop, and α-naphthalene acetic acid in particular is now widely used by growers for reducing the pre-harvest drop of apples and pears.

The importance of the developing seeds as centres of growth-hormone production has already been stressed, and the question therefore arises as to what part, if any, this seed hormone plays in the prevention of fruit-drop. Heinicke [14] found that apples with the fewest seeds tended to drop the most readily, which led him to suggest that the developing seeds have a pulling power for water and sap enabling those fruits with the greater number of seeds to develop at the expense of other fruits with smaller food-attracting abilities. In the light of modern knowledge, a more probable explanation seems to be that such fruits remain attached because, on account of their higher seed content, they produce more growth-hormone than fruits with fewer seeds. This supposition has recently been strengthened by direct measurements of the hormone content of seeds from dropped fruitlets and attached fruitlets in a number of apple varieties. Not only do the former contain fewer seeds, but the hormone content per seed is lower than in those fruits which do not fall [5].

There appears also to be some correlation between the variations in hormone content of apple seeds throughout the season and the successive waves of fruit-drop, particularly during the early part of the season [5]. In the varieties so far investigated, the main peaks of hormone production are found to coincide with 'sticking periods' when few fruits fall from the tree; conversely, periods of heavy fruit-drop are associated with low hormone production by the seeds (figure 10).

The wide fluctuations in hormone content throughout the season are themselves closely correlated with developmental changes within the seed, particularly with the growth of the endosperm, which, as already mentioned, is the chief hormone-producing tissue of the seed. The apple and related fruits are unusual in that the endosperm remains in a free nuclear condition for a period of some three to four weeks following fertilization (figure 8). During this period the seeds manufacture no hormone and abscission readily occurs. This first wave of fruit-drop is terminated by the formation of a cellular endosperm tissue (figure 8 b) and the simultaneous production of hormone. Subsequent partial digestion of the developing endosperm by the rapidly growing embryo causes a further temporary fall in hormone production which, in some varieties, seems to be associated with the June drop. On the completion of embryo growth, hormone production again rises and fruit-drop ceases.

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Electroencephalography

W. GREY WALTER

The human brain is more complex than any other known structure in the universe, and science can still do little except grope at unravelling its complexity. However, modern work on radar and the like has opened up a promising line of approach to the subject, and a new branch of science, electroencephalography, has been born. Dr Grey Walter, a pioneer in this field, describes the latest advances, and shows how some of the results may be interpreted.

The classical philosophers took so little interest in the brain that they referred to it merely as 'the thing in the head.' Until the last century it was considered as providing a sort of radiator for overheated animal spirits; only in the last generation has the study of brain-function developed into a serious science. Microscopical examination of the brain tissue revealed that it contains about 10^{10} nerve cells arranged in complex and systematic three-dimensional patterns. Electrical stimulation of the exposed brain and observation of the resulting movements and subjective sensations demonstrated the anatomical connection between some parts of the brain and various regions of the body, and it was found that, even when inactive, the brain consumes an enormous quantity of energy in relation to its size. Only in the last twenty years, however, has it been possible to study any aspect of brain function directly in the intact human subject. It had been known for a long time that communication between individual nerve cells is maintained by brief electro-chemical discharges along nerve fibres, but it was not believed that the electrical activity of the brain could be detected without placing electrodes directly on the exposed nerve-tissue until Berger demonstrated this possibility in 1928. A record of the electrical brain activity obtained in this way with electrodes on the scalp is called the electroencephalogram or E.E.G.

Such records show continuous electrical activity of an extremely complex nature, consonant with the huge number of nerve cells and the intricacy of behaviour patterns. As recorded through the scalp and skull, the potential differences of the electrical discharges are only a few millionths of a volt and have to be amplified with specially designed electronic devices which convert them into a continuous graph drawn on paper. The possibility of studying the normal brain in this way encouraged both clinical developments and the theoretical consideration of brain function as a whole. Present concepts can be summarized most concisely by stating that the general function of the brain is to construct and contain a working model of the outside world, and to test upon this model the effect of the operations which circumstances suggest are necessary for comfort or survival. The more accurate and detailed the model, the more trustworthy will be the forecast of the results of action, and the better the chances of survival of the organism. In the human brain, the working parts of the model are the circuits which link the nerve cells; since their permutations are of the order of $10^{10,000}$ there seems ample scope for detail—and for fantasy.

The notion of the brain as a signal analyser and statistical predictor has developed parallel with the design and construction of the various large computing engines sometimes known as electronic brains, but it must be admitted that as yet very little is known of the precise manner in which the animal brain composes its models so that they are compact enough to be portable, plastic enough to be changed from second to second, yet durable enough for a lifetime. It may be mentioned, however, that in man the purely nervous model is totally inadequate for social life, and is supplemented by external patterns in the form of written records, laws, and so forth. The neurophysiologist, then, would use the term 'mind' as meaning the individual's model of his environment and 'thought' as a miniature rehearsal for action. Such generalizations are bound to reawaken many ancient philosophical controversies, but have the virtue of promising linkage between brain physiology and other human interests, some of which will be considered later.

Returning to the discoveries in the field of electroencephalography, it was found at an early stage that recognizable perturbations of the electrical patterns occur in diseases of the brain. This observation had the unfortunate effect of distracting

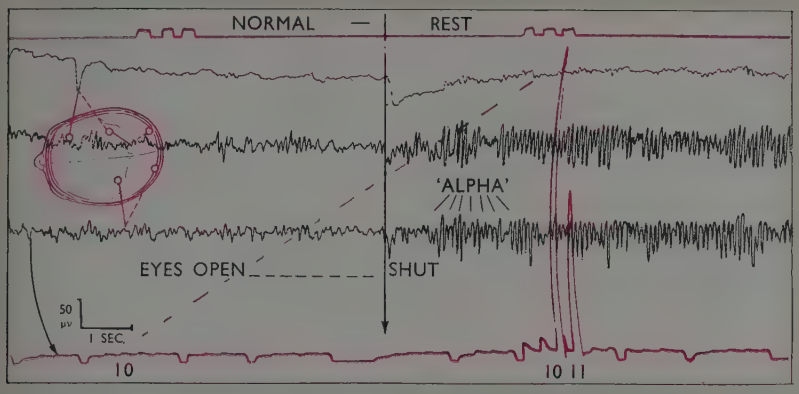

FIGURE 1 *a* – *Electroencephalogram from a normal person showing the effect of closing the eyes. Note the increase in alpha activity at* 10 *cycles per second in both occipital regions and the associated peak at* 10 *in the frequency analysis (shown in red).*

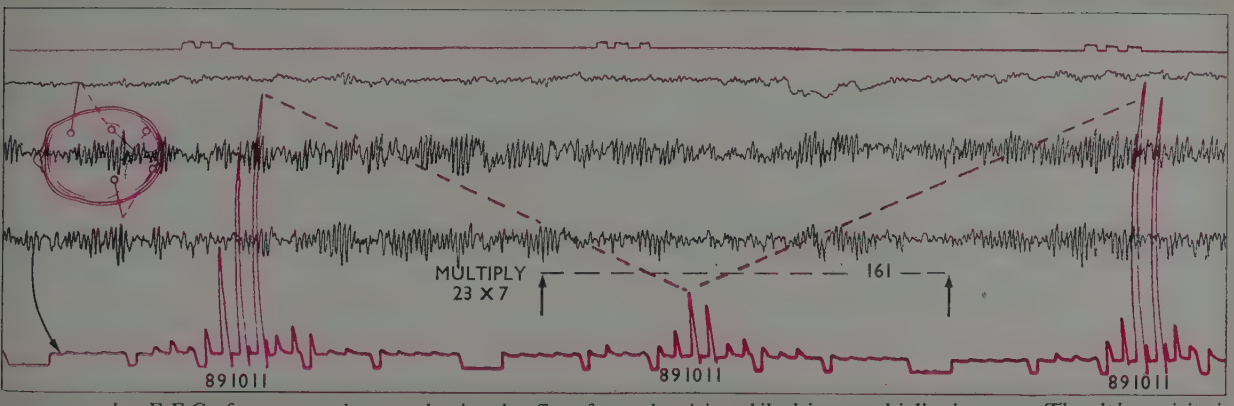

FIGURE 1 *b* – *E.E.G. from a normal person showing the effect of mental activity while doing a multiplication sum. The alpha activity is reduced and the extent of the reduction is indicated by the smaller size of the peaks at* 8, 9, 10, *and* 11 *cycles per second in the analysis.*

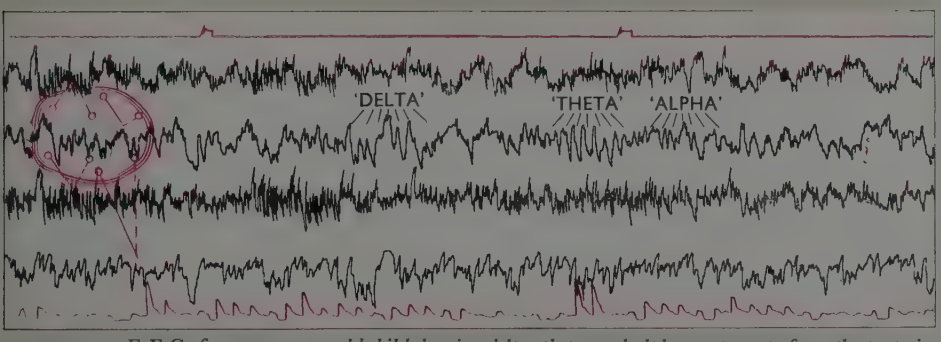

FIGURE 2 – *E.E.G. from a one-year-old child showing delta, theta, and alpha components from the posterior regions. The analysis is from the frontal areas and demonstrates the absence of consistent rhythmic activity.*

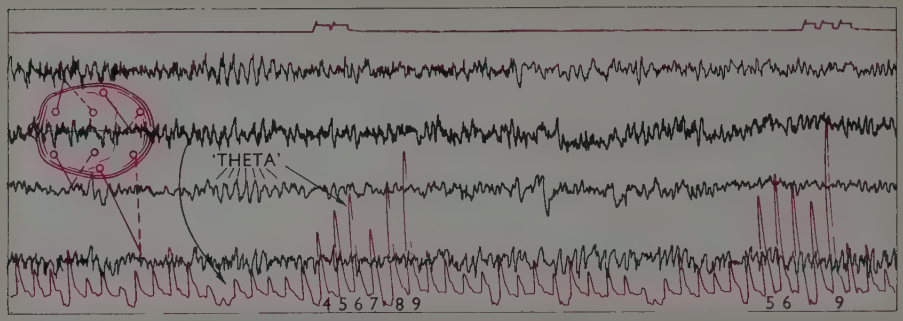

FIGURE 3 – *E.E.G. from a three-year-old child showing prominent theta activity combined with alpha. A similar record might be found in an adult with marked aggressive tendencies.*

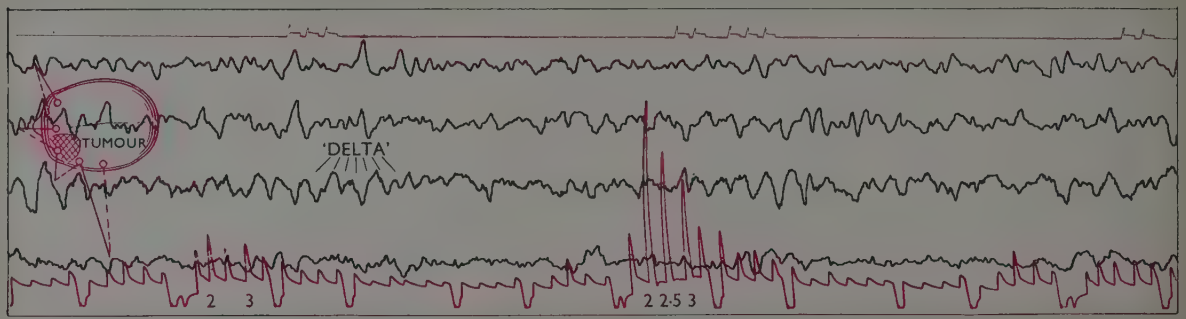

FIGURE 4 – *E.E.G. from a patient with a large tumour in the left frontal region which is generating the large slow delta waves. Note the peaks at* 2, 2·5, *and* 3 *in the analysis from between channels* 2 *and* 3 *showing where the slow waves are coming from.*

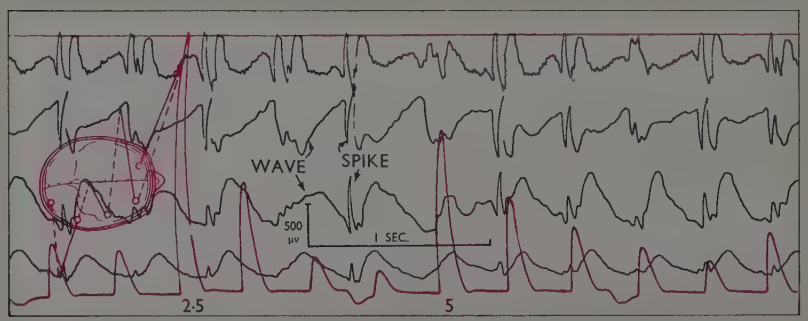

FIGURE 5 – *E.E.G. taken during a minor epileptic seizure showing the prominent 'wave and spike' activity diagnostic of this condition. Note that the time scale is longer than in the other records and that the amplification has been greatly reduced.*

attention from fundamental problems, so that most laboratories engaged in this sort of work were soon full of records from epileptics and patients with brain tumours, and, during the war, with head injuries and abscesses. The literature of the subject contains thousands of papers and reports describing empirical correlations between various E.E.G. features and brain diseases, but of these publications only a few dozen are of fundamental importance, and there is no doubt that the great discoveries are still to be made.

Records from normal people show a great variety of more or less irregular electrical discharges from all parts of the brain, but in most subjects, when the eyes are shut, the discharges become more rhythmic in the occipital region, where visual impressions are projected. These regular oscillations are known as alpha rhythms and have a frequency of between 8 and 13 cycles per second. Even with the eyes shut, some alpha rhythms are usually diminished when the subject thinks hard, and some are blocked when a vivid visual image is called up. There is wide variation between individuals, but the general picture in a given subject is very constant and is almost like a signature (figure 1). It is important to note the paradoxical inverse relation between the prominence of the alpha rhythms and the level of functional activity and attention. In babies, there is little alpha activity; there are only much slower swings of electric potential in all areas (figure 2). In older children, rhythms at about 6 cycles per second are prominent up to the age of 7 or 8 (figure 3), but the alpha rhythms are beginning to appear, and the adult pattern is usually established by the age of 12. The 6 cycles per second activity is sometimes called theta rhythm; it is often larger when the child is unhappy or angry. It comes from the temporal lobes of the brain and deeper regions, and is found in many adults whose behaviour is childish in the sense that they are short-tempered, aggressive, and hard to get on with.

In sleep, the alpha rhythms disappear first, and the E.E.G. becomes less individual. In deep sleep it resembles a baby's record, but in lighter sleep there is usually intermittent activity at 14 cycles per second, and this is sometimes prominent just before a subject wakes up from a dream. Hypnosis produces little change unless the subject is of the type who goes into a very deep trance, when the alpha rhythms are said by some to respond to the suggested, rather than the real, conditions.

When the brain is injured, or invaded by a tumour, the affected area tends to develop slow discharges called delta activity (figure 4). Although they are usually localized round the lesion, these slow, abnormal delta waves are quite similar to those found in infants and deep sleep, suggesting that they may have some function in assisting the disturbed nervous tissue to rest from its labours, in the same way as pain compels disuse of a broken limb—for the brain can feel no pain, and has little power of healing itself.

The most dramatic variations from the normal are seen in epileptics, particularly during seizures. In *Petit Mal*, for example, when the patient suddenly becomes unconscious for a short time, the whole brain generates enormous regular slow waves a hundred times bigger than any normal rhythm, each one with a short spike indenting its crest (figure 5). One may consider a patient generating these 'wave and spike' rhythms, as they are called, as being electrocuted by his own brain.

Though always interesting and sometimes useful in the study of organic brain disease and epilepsy, the technique of electroencephalography has been too crude to give much help in problems of mental disorder, or in the investigation of the physiology of behaviour. In the last few years, however, the technical resources available to electro-physiologists have been greatly extended by the adoption and adaptation of devices developed during the war for radar or 'servo' machinery. Some new instruments, on the other hand, have been specially designed by those working in the field for their own peculiar problems.

The advance has been made on two fronts: first, the development of elaborate and flexible methods of transformation and display of the electrical data; second, the introduction of controlled stimulation of the subject. It will be realized that the ordinary record is really a graph of voltage against time, but since there are inevitably large numbers of cell-groups active at the same moment, this graph is nearly always extremely complex and bewildering to the eye, which can pick out only one or two of its many components. An instrument has been designed, and is now in use in many laboratories, which automatically breaks down the complex oscillations into their various constituents, rather as a spectroscope resolves white light into its component colours. This wave analyser writes out on the ordinary record every ten seconds a frequency histogram corresponding to the Fourier transformation of the primary changes. A mathematician would take several days to perform the same

task. Another device, still under development, displays the electrical activity on a battery of twenty-four cathode-ray tubes, each one corresponding to a small area of the brain. In this 'toposcope,' voltage is transformed into the brilliance of the cathode-ray screen, and an indication of frequency is given by the way in which the patch of light seems to rotate on each tube.

Ten years ago these devices would have seemed absurdly elaborate: in another ten year's time they will probably seem childishly simple compared with the intricacy of brain function as represented by the E.E.G. Even now, we can probably understand less than 1 per cent. of the total information contained in a record such as those in the figures. We are rather in the position of a visitor from Mars who is deaf and dumb and has no conception of the nature of sound, but is trying to build up a knowledge of languages by looking at the grooves on a gramophone record.

In dealing with a complex signal, there is always the problem of deciding how the significant parts of the message can be made most clear and memorable and the insignificant ones least obtrusive. Success in this discrimination depends upon choosing the right criteria for significance, and this can often be done only empirically until the cipher has been broken, so to speak.

The second sign of progress is that a more active approach is being made to the brain by providing controlled stimuli instead of merely watching its spontaneous activity. An analogy for this is the development of radar, in which instead of trying to pick up the sound or radiation from an aeroplane, a radio pulse is transmitted towards it and the echoes produced are observed in relation to the original signal. In the study of the brain the transmitted signals are called stimuli and the echoes responses, but the resemblance in both method and results is very close. The sound from an aeroplane travels too slowly to indicate the position of the source, and any radio signals it emits may be deliberately or inadvertently confusing to the observer, but an aircraft cannot avoid reflecting the radar pulse. In the brain, the spontaneous actions or thoughts which it initiates may be delayed, by a variable time, after the first electrical sign of their occurrence, and the spontaneous activity is usually too varied and uncontrollable to correlate with other physiological variables, but the central nervous system cannot escape the influence of external stimuli, since to respond to them is one of its prime functions. Moreover the stimuli can be given at moments controllable by the observer and at known frequencies.

By using techniques precisely similar to those developed for radar, the response to a regular stimulus can be displayed on a cathode-ray oscilloscope, so that scores of successive responses are precisely superimposed—whereas the random or spontaneous activity is blurred and unobtrusive. This method has been turned to great account by Dawson [1] in the study of the electrical responses to stimulation of sensory nerves in the limbs. Results of considerable general interest have been obtained by using rhythmic flashes of light for investigating the response-characteristics of the visual cortex [2], [3], [4]. When rhythmic stimulation of this sort is used, the frequency analyser already mentioned is of great value, since the regular responses at the stimulus frequency can be traced through the brain, even when they are smaller than the irregular background oscillations, because their steady recurrence is seen by the analyser as a prominent peak of activity at a single frequency.

This method has proved powerful in clinical applications, particularly when the resting records present no diagnostic features. In some epileptics, for example, it is possible to find a flash-frequency which sets up in the brain electrical activity which combines with the oscillations already present to produce a diagnostically abnormal pattern, and a clinical seizure. This observation, in combination with the known features of the spectrum of epileptic records, has suggested the hypothesis that the condition known as idiopathic epilepsy is due to the occasional synchronization by sensory or motor impulses of otherwise unrelated electrical rhythms. In cases with organic disease of the brain the response to rhythmic flashes of light is often disturbed near the site of a lesion, even when the resting activity appears normal.

In normal subjects, bright rhythmic flashes of light have been found to evoke peculiar sensations at certain frequencies. All subjects describe chequered, whirling patterns of light and shade when the stimulus frequency is between 7 and 30 flashes per second, and most see brilliant and ever-changing colours with white light. Some people, again, describe vivid hallucinations and dream-like experiences of flying or of distortion of the time sense. Most interestingly, whenever a peculiar sensation is experienced, some part of the brain displays at the same time an unusual and exaggerated degree of electrical activity at the frequency of the stimulus or a multiple of it. Furthermore, when,

during an episode of this sort, the subject is encouraged to submit to the sensation and to reinforce it with memories of a similar type, both the sensation and its associated electrical discharge are augmented. Conversely, when the subject is distracted by another stimulus or, by an effort of will, refuses to accept the vision or state of mind induced by the stimulus, both the subjective and electrical phenomena subside.

In some of the most striking examples of this effect, vivid and emotionally powerful feelings have been correlated with electrical responses in the temporal and frontal lobes, not at the stimulus frequency itself, but at harmonics of it. In general terms, complex individual mental disturbances can be set up by an apparently neutral physiological stimulus when this is rhythmic and powerful enough, and these mental states are strictly correlated with electrical events in parts of the brain which have nothing to do with the reception of visual stimuli.

It will be recalled that unhappy children and bad-tempered adults often show discharges at about 6 cycles per second in the temporal lobes. It is at least very suggestive that when similar electrical discharges are evoked in normal people by rhythmic stimulation, a feeling of emotional discomfort and irritability appears. It would seem logical to adopt, as a working hypothesis, the notion that certain temperaments and states of mind are associated with electrical activity at a particular frequency in certain nervous circuits within the brain, and one can foresee a new meaning of the word 'temper' as a strictly scientific term to describe the tuning and resonance of these circuits. In the testing of working hypotheses 'the road of excess leads to the palace of wisdom,' and it may be necessary to develop a complete theory of the relation between brain and mind in these electrical terms before the appearance of absurd conclusions or predictions signals the inadequacy of our ideas. Even at the present time the introduction of electrical terms and analogies has accelerated advance, for it is now possible to use the methods of electrical engineers in the study of brain physiology. For example, when an engineer observes a sustained electrical oscillation he looks for what he calls 'positive feedback' and it seems likely that, in the brain, feedback circuits actually exist, and that their properties can be studied exactly as if the living organ were a complex transmission system. Going a step further, it has been found possible to construct models using devices similar to those postulated in the brain, and these models behave in a very lifelike fashion, particularly when positive and negative feedback circuits are combined. The positive feedbacks provide mechanisms for hunting, scanning, and testing the model's environment for stimuli and information, and the negative feedbacks or reflexes ensure that any action the model may decide to take will be as near as possible to that necessary for stability and survival. It is surprising how complex and apparently unpredictable the behaviour of one of these models can be when it is placed in the irregular environment of an ordinary room, even when it contains only half a dozen units as compared with the millions in the human brain.

The great fertility of the marriage between brain physiology and engineering has suggested to many people that the family of subjects thus united is worthy of a special name, and Wiener [5] has proposed 'cybernetics,' from the Greek word meaning 'steersmanship,' since the fundamental common problem seems to be the way in which complex dynamic systems direct themselves toward various goals. It has been predicted that when properly established this new branch of science may have as great an effect upon our life and surroundings as the achievements of nuclear physicists.

A comparison has already been made with the great calculating machines, but these are of course enormously more specialized than any viable living organism, since their sole function is to perform certain types of calculation at immense speed. In contrast to this, the brain performs innumerable functions, though none with very great accuracy or velocity—but it can design the machines. A good-quality human brain has more possibilities than any other known structure in the universe, and we should not be far wrong in calling it 'the universal machine.'

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Book reviews

THEORETICAL AND INORGANIC CHEMISTRY

A Text Book of Theoretical and Inorganic Chemistry, *by F. A. Philbrick and E. J. Holmyard. Revised in collaboration with W. G. Palmer. Pp. v + 835, with numerous line diagrams. J. M. Dent and Sons Limited, London.* 1949. 13*s.* 6*d. net.*

Seventeen years have elapsed since Philbrick and Holmyard's *Text Book of Theoretical and Inorganic Chemistry* first appeared. It has enjoyed wide popularity, and now the authors have prepared a new edition. The new work retains the general plan of the original text, but much of the matter has been rewritten to bring it into line with modern ideas and to incorporate recent advances in knowledge. As Mr Philbrick was unable to take an active part in the revision, Dr Holmyard secured the collaboration of Dr W. G. Palmer of Cambridge, whose valuable book on valency is well known to chemists. Part I serves as an excellent historical introduction, while Part II deals with general and theoretical chemistry. Here are discussed with lucidity and conciseness such topics as the structure of matter, the nature of solution, heterogeneous equilibrium and homogeneous equilibrium, electrochemistry and photochemistry, colloid and surface chemistry, radioactivity and atomic structure, and finally the theory of valency. In Part III the elements and their compounds are considered, the less common elements included. This new volume is admirably written and well illustrated, and bears evidence on every page of the hand of the experienced teacher. There is no doubt it will be even more successful than the original volume. Considering the size of the book and the attractive way in which it is produced the price is most reasonable. Candidates for pass and honours degrees and for university entrance scholarships should be sure to have this book. They will find it a good companion. W. WARDLAW

ATMOSPHERIC ELECTRICITY

Atmospheric Electricity, *by J. Alan Chalmers. Pp.* 175, *with several line diagrams. Oxford University Press, London.* 1949. 15*s. net.*

Our knowledge of atmospheric electricity has been largely developed during the last fifty years by a band of enthusiasts, foremost amongst whom have been Elster and Geitel, C. T. R. Wilson, G. C. Simpson, and B. F. J. Schonland. Apart from a small monograph by the last named, no English text has been devoted to the subject. Dr Chalmers' book is claimed to fill this gap (wisely omitting the daughter subjects of ionospheric structure and cosmic rays, now quite beyond parental control), and to serve both as an introduction to the uninitiated and as a work of reference. This is difficult enough in 175 pages, but Dr Chalmers makes it quite impossible by assuming in his reader not even a knowledge of elementary electrostatics. He introduces laboured explanations involving lines of force for almost every application of Poisson's equation or Gauss's theorem, and is left with too little space for critical discussion of modern work.

The book's greatest value is as a source of references. Some 250 are listed, the majority being abstracted in the text. They carry the story up to 1947, apparently without important omission, except possibly of the recent work of J. Frenkel. G. D. ROBINSON

REFLEX FACTORS

Conditioned Reflexes and Neuron Organization, *by Jerzy Konorski. Pp.* 267, *with* 18 *line diagrams. Cambridge University Press, Cambridge.* 1948. 18*s. net.*

This is an attempt to resolve highly integrated animal behaviour into reflex factors. Such resolution is extremely desirable, and the attempt is courageously conceived and executed. The field undertaken is the behaviour of the laboratory dog, and the observations proceed on the lines introduced by the great Russian observer, Ivan Pavlov, but the interpretation put upon them constitutes a great advance. Professor Konorski worked with Pavlov for a number of years, and after Pavlov's death continued independently the line of inquiry followed in the Leningrad Institute. The prosecution of patient scientific research was at the time of the preparation of this book in itself a notable achievement.

The observational basis of the work consists of the experiments due to Pavlov, and a long series of further ones by Professor Konorski and his Polish and Russian colleagues. These are now, in their English dress, made available to a fresh audience. It will be remembered that Pavlov expressed himself as not wholly satisfied with his own interpretation of some of the experimental results he obtained. Particularly in his later years, his explanations had to call in what he called the different 'temperaments' for some of his individual dogs. Professor Konorski reworking over the subject now furnishes an amended and more strictly physiological explanation of the 'conditioned reflex' as exemplified. This type of animal 'behaviour,' if we go back to the Hughlings Jackson paradigm, is a reaction belonging to the highest type of the three Jacksonian levels. It is commingled therefore with the mental and the psychical. That is a welcome fact about it today, because it is precisely that type of behaviour which has, as regards experimental inquiry, lain rather in a backwater in recent years. In Europe, though less in America, it has suffered from falling between two stools, neurology and psychology. Professor Konorski is to be congratulated on his valuable monograph.

C. S. SHERRINGTON

A HISTORY OF THE ROYAL SOCIETY

Scientists and Amateurs, *by Dorothy Stimson. Pp. xiii + 270. Sigma Books Limited, London.* 1949. 15*s. net.*

The author, who is chairman of the history department at Boucher College, Baltimore, states in her preface: 'Those who have the time and the opportunity to look through the Royal Society's own *Record* or to refer to its treasurer's recent careful study of its administration for nearly three centuries may not need this book. But copies of neither book are readily available to an American public because of cost or scarcity, nor is either one planned for the general reader who knows little or nothing of the Royal Society at the outset.'

Miss Stimson has succeeded admirably in this attempt to interest the general reader in the life-story of one of the world's oldest scientific bodies. Her book is scholarly, authoritative, and comprehensive; its style is excellently adapted to its object; its illustrations are well chosen and beautifully reproduced. Scientists and amateurs alike will discover that this is a volume difficult not to finish at one sitting; the British, as well as the American, public will find it full of fascination.

JAMES KENDALL

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Was born in London in 1897, and was educated at Westminster City School and Balliol College, Oxford. He was a Fellow of Balliol, 1920–1, and Fellow and Tutor of Trinity College, Oxford, 1921–37. At present he is Dr Lee's Professor of Chemistry in the University of Oxford, and a Fellow of Exeter College. He was president of The Chemical Society, 1946–8, and is a member of the Fuel Research Board. He has written books on the kinetics of chemical change and the chemical kinetics of the bacterial cell. He is an honorary member of many foreign scientific societies, and has received the Davy and Royal medals of the Royal Society, the Lavoisier medal of the *Société Chimique de France*, and other awards.

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Was born at Brisbane, Australia, in 1915, and was educated at Geelong Grammar School and the Universities of Melbourne and Cambridge. He has carried out research on interspecific competition in insects, with a view to putting this aspect of the theory of natural selection on a more quantitative basis. In 1946 he was appointed a lecturer in the Department of History and Philosophy of Science at University College, London.

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Was born in 1917. He has worked on high-frequency electrical discharges under Professor Sir John Townsend at Oxford, and on certain problems of glass physics under Professor W. E. S. Turner at Sheffield. In 1942 he returned to Oxford to join Professor F. E. Simon, and until 1946 took part in the Government's atomic energy programme. He has since been a member of Professor Simon's Low Temperature Department at the Clarendon Laboratory, and has been working on problems of solid-liquid equilibria. He has lately been appointed Reader in Experimental Physics in the University of London, at Queen Mary College.

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Born in Berlin 1893, educated at Munich, Göttingen, and Berlin Universities. Was Professor Extraordinary at Berlin from 1927, Professor Ordinary and Director of the Laboratory of Physical Chemistry at Breslau in 1931, and Visiting Professor at Berkeley, California, in 1932. He left Germany in 1933 and settled at Oxford, where he is now Professor of Thermodynamics and a Student of Christ Church. During the war he was a member of the technical committee in charge of the British Atomic Energy project. In 1948 he was awarded the Rumford Medal of the Royal Society.

His main field of interest is the physics of very low temperatures. His earlier work was mainly concerned with the elucidation of the correct meaning of Nernst's Theorem. Later his work extended to cover most problems in the low-temperature field, in particular the properties of solid and liquid helium, the discovery (with Rollin) of the 'creeping film,' and investigations below 1° in the region attained with the magnetic method. At present he is in charge of the new Low Temperature Department of the Clarendon Laboratory. He is Chairman of the Low Temperature Group of the Physical Society and President of the recently formed Commission for Very Low Temperatures of the *Union de Physique*.

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Was born in 1914 and was educated at Cotham School, Bristol, and at the University of Bristol. In 1938 he joined the botanical staff of Manchester University and later held a similar appointment at Aberdeen University. During the war he was a member of the Operational Research Station at Bomber Command. Since 1945 he has held the post of research pomologist at the Agricultural and Horticultural Station, Long Ashton, Somerset.

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Born 1910, Kansas City, Missouri, U.S.A., of English parentage; returned to England 1917. Educated Westminster and King's College, Cambridge. Carried out research in electro-physiology under Adrian and Matthews until 1935. Rockefeller Fellow, Central Laboratory, Maudsley Hospital, from 1935 to 1939, studying human electrophysiology, especially E.E.G. under Golla; discovered and named delta waves and location of brain tumours. Appointed Director of the Physiological Department, Burden Neurological Institute, Bristol, 1939. Invented wave analyser, 1943. With assistant V. J. Dovey (later wife) discovered theta rhythm and electrical method of tumour delimitation during operations, and introduced rhythmic stimulation with analysis as aid to clinical diagnosis, 1946. Founded E.E.G. Society 1942. Is co-founder and co-editor of international journal *Electroencephalography and Clinical Neurophysiology*.

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